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MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

VOL. LXXVII.

EDITED BY

JAMES FORREST, ASSOC. INST. C.E., SECRETARY.

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ERRATA.

Vol. lxxv., p. 247, line 3, *for* "Joseph" *read* "Julius."
" lxxvi., p. 355, " 10, *for* "May" *read* "Mayo."
" " p. 356, " 16 from bottom, *for* a full stop *substitute* a comma.

THE
INSTITUTION
OF
CIVIL ENGINEERS.

SESSION 1883-84.—PART III.

SECT. I.—MINUTES OF PROCEEDINGS.

26 February, 1884.

Sir J. W. BAZALGETTE, C.B., President,
in the Chair.

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(*Paper No. 1968.*)

"Hydraulic Propulsion."

By SYDNEY WALKER BARNABY, Assoc. M. Inst. C.E.

So much has been said and written on the subject of hydraulic propulsion, and the principles underlying it are so well understood, that the Author proposes, notwithstanding the title of the Paper, to give little more than a description of the latest boat propelled by this system, and a comparison of it with a sister-boat driven by a screw, and with some hydraulic vessels built previously.

The first mention of this system of propulsion which the Author is able to find is in an old patent taken out in the year 1661 by Toogood and Hayes, for "a particuler way of Forceing Water through the Bottome or Sides of Shipps belowe the Surface or Toppe of the Water, which may bee of singuler Vse and ease in navigac̃on." Many patents were subsequently obtained, and Mr. Ruthven, whose patent is dated 1839, built two vessels, one vessel 9 feet long, which was tried in Edinburgh, and the other 40 feet long tried on the Forth in 1844. A Vessel for commercial purposes, fitted with Ruthven's propeller was built in Prussia in 1853; and, besides others, a floating fire-engine was constructed on the Thames, in which, by the advice of the late I. K. Brunel, the pumping power was utilized for propelling the vessel, an adaptation of the turbine propeller for which it is specially suited.

Public attention was first prominently directed to the claims of this propeller by the trial of the "Nautilus" in 1866. This vessel, which was 115 feet long, and had engines of 127 indicated HP., attained a speed of 8·32 knots per hour. She was propelled by a turbine 7 feet in diameter, which drew water from an opening in the bottom of the boat near the fore part, and discharged it through two nozzles in the sides, just above the water. The area of each nozzle was 78·54 square inches.

In 1866 also the "Waterwitch," an armoured gun-boat, 162 feet long, 32 feet beam, and having 1,161 tons displacement, was built for the Admiralty at the Thames Ironworks, the machinery being designed by Ruthven, and constructed by Messrs. Dudgeon at Millwall. She was driven by two water-jets, discharged from nozzles at the sides level with the water, the mean diameter of each of which was 24 inches. The speed attained by the vessel was 9·3 knots per hour. This ship was sister to the "Viper," a twin screw-ship, but the latter suffered from the disadvantage of a double keel aft and a slightly fuller run. A detailed comparison between the two will be found in Table 1, Appendix II.

In 1878, a hydraulic torpedo vessel was built by the Swedish Government for competition with a similar vessel with twin screws; and by the kindness of her designer, Mr. Lilliehöök, the Author is able to give drawings of her arrangements. Figs. 1 and 2 show the position of the pumps, and of the inlets and outlets. In this vessel two pumps were used, not because the designer thought it an advantage, but in order that a passage might be maintained from the forward to the after part of the ship below the deck. The performance of this vessel will likewise be found in Table 1, Appendix II.

In 1879, the "Fleischer Hydromotor,"[1] a ship of a different design, but still propelled by the reaction of jets of water, was built in Germany. In this vessel the water is acted upon directly by steam without the intervention of a pump. The arrangement is as follows:—There is a cylinder lined inside with wood, at the bottom of which is a large pipe leading to a nozzle at the bottom of the vessel. A float, of nearly the same diameter as the cylinder, works up and down in it. The cylinder being now full of water, and the float, consequently at the top, steam is admitted by a valve above the float and driving it down, ejects the water through the nozzle. On reaching the bottom of its stroke, the float opens the exhaust, and the steam passes into the condenser. The vacuum thus created in the cylinder causes the water to rise partly through the nozzle, but principally through a suction-valve in the bottom of the condenser. The cylinder is thus filled with water, and the float rises to the top, in doing which it closes the exhaust and opens the steam-valve, when the operation is repeated. The loss by condensation appears, from the indicator cards, to have been much less than might have been expected in a cylinder filled alternately with steam and with water; but as the cylinder is not entirely emptied at each stroke, a layer of boiling water always remains at the top and adheres to the wooden lining as the float descends. The vibration was enormous, and the jerky motion was very unpleasant at the earlier trials, but was somewhat

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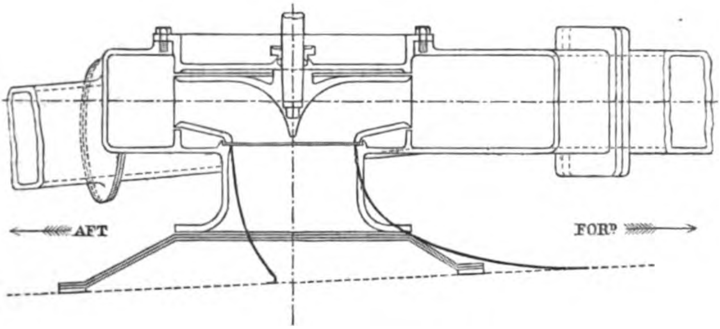
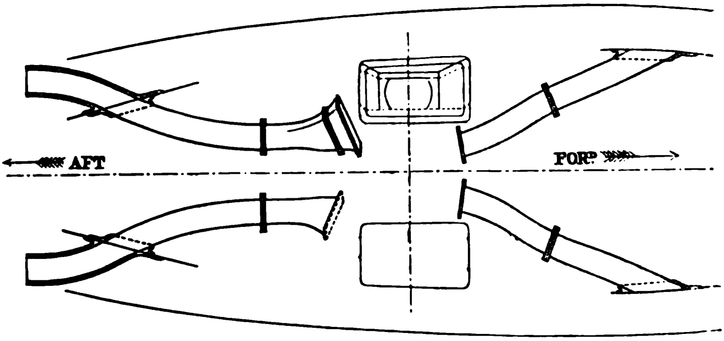
[1] A brief description of this propeller was given in the Minutes of Proceedings Inst. C.E., vol. lix., p. 408.—SEC. INST. C.E.

FIG. 1.



LONGITUDINAL SECTION THROUGH INLET OF SWEDISH HYDRAULIC VESSEL.

FIG. 2.



PLAN OF DISCHARGE-PIPES OF SWEDISH HYDRAULIC VESSEL.

lessened afterwards. The data obtainable are unreliable, as the speed is variously given as 9 and as 6 knots per hour, and there is no means of ascertaining the loss between the boiler and the indicator, which is probably large. As the down stroke is made by the pressure of the steam and the upstroke by the pressure of the atmosphere, it is probable that the upstroke occupies the longer time. Water is ejected and propelling takes place during the down stroke; therefore, in order to eject a given volume of water per second at a given mean velocity of discharge, it is necessary either to have two or more pumps where one would be sufficient if the action were continuous, or the actual velocity of discharge must be at least double, and probably, more than double, the mean velocity. Either plan produces loss of efficiency.

The advantages claimed by the advocates of the hydraulic system of propulsion may be enumerated as follow:—No impediment to speed under sail; no racing due to pitching; no vibration; power of reversing-motion in the hands of the officer on deck; full engine-power for manœuvring; vessel capable of being made double-ended, and power of ramming much increased. The propeller is not liable to damage from running aground, and cannot be fouled by floating obstructions; it is favourable for light draught; and the large pumping power is available for keeping down leaks. The disadvantages of the hydraulic propeller are mainly these: First and foremost is the difficulty of utilizing the full energy of the feed-water, or the velocity of the water entering the propeller. Secondly, every particle of water acted on must be carried in the ship. Thirdly, the loss by friction of the water in the passages. Fourthly, the loss by bends in the pipe, which can only be reduced by increasing the losses from the second and third causes, namely, the weight of water carried, and the friction of the passages.

The advocates of the system of hydraulic propulsion were not satisfied with the "Waterwitch" as an embodiment of their ideas. They were supported in their demands for a further trial by Lord Dufferin's Committee on Designs in 1871. This Committee, which numbered amongst its members Sir William Thomson, Professor Rankine, and Mr. Froude, inserted the following clause in their report:—

"Our attention has been directed to the hydraulic method of propelling steam-ships, with reference especially to vessels of very light draught, and intended for service in waters which are so shallow as scarcely to afford sufficient immersion even for twin screws, or in which there is reason to apprehend that the screws are likely to be fouled by obstacles placed there for that purpose. Regarded in this light, we are of opinion that the system is deserving of a more thorough trial than it has yet received, and we beg leave to recommend the subject to their Lordships' consideration accordingly."[1] The Admiralty hesitated long before doing so, but were finally induced in 1881 to order one of a number of second-class torpedo-boats, then being built by Messrs. Thornycroft, to be fitted with a turbine-propeller. It was intended that the boat should be as much as possible like one of the screw-boats, in order that the performance of the two might be compared. It was, however, soon found desirable to make certain modifications in the form of the hydraulic-boat, the reasons for which will be explained. The conditions laid down by the Admiralty were that a similar boiler was to be used to that in the screw boat, and about the same HP. developed, and that the builders were to obtain as much speed as they could.

The number of revolutions made by the engines of the screw-boats are about six hundred and thirty per minute. This was considered much too high a velocity for the turbine, and its revolutions were fixed at four hundred per minute. This involved larger cylinders. Those of the screw-engines were 8¼ inches and 13½ inches in diameter and 8 inches length of stroke; while those of the hydraulic engines were increased to 8½ inches and 14½ inches in diameter and 12 inches length of stroke; and, although the latter were relieved of the weight of reversing gear, they were still considerably the heavier, the former being 1·9 ton, and the latter, including pumps and water in pipes, 2·58 tons. The total weight of machinery, including water in the boiler, &c., is, for the screw, 4·87 tons; for the turbine, 5·56 tons.

It was felt that a vessel of the same dimensions as the screw-boat would be much handicapped by this extra weight, as the whole displacement of the latter is only 12·9 tons. The length was therefore increased from 63 feet to 66 feet 4 inches, and this, together with a modification of the bottom, which gave some extra displacement, restored the boat to about the same draught amidships as the screw type.

The difference observable in the disposition of the machinery was dictated by the necessities of trim. This explains why the boat, intended for comparison with a similar vessel driven by a screw, was not made of the same dimensions. The designers consider that the alterations render the conditions much more fair for the hydraulic boat than if no modification had been made. The difference in length tells slightly in favour of the longer boat.

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[1] Report of the Committee appointed by the Lords Commissioners of the Admiralty to examine the designs upon which ships of war have recently been constructed, 1872, p. xix.

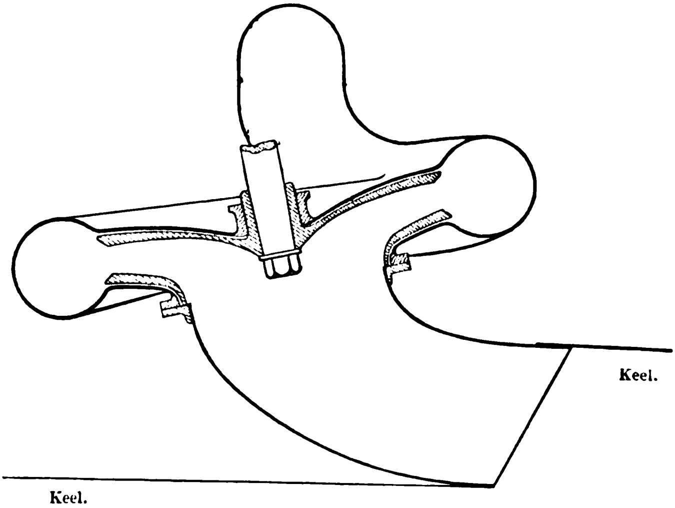
It will be convenient here to glance at the causes of loss of work in propellers of different kinds, and the methods adopted for reducing them to a minimum in the boat under consideration. These causes of loss, irrespective of friction, may be thus summed up:—

1st. Suddenness of change from velocity of feed to velocity of discharge.

2nd. Transverse motion impressed on the water.

3rd. Waste of energy of the feed-water.

FIG. 3.

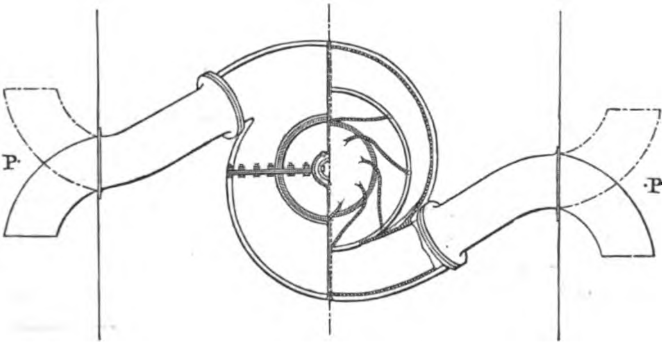


Propellers which suffer from the first cause of loss are, the ordinary uniform-pitch screw and the ordinary paddle-wheel; while those which in varying degrees avoid it are, the gaining-pitch screw, certain forms of feathering paddles, Ruthven's form of centrifugal pump, and, probably best of all, the oar. Propellers which undergo loss from the second cause, namely, transverse motion imparted to the water, are, ordinary screw-propellers, radial paddle-wheels, and oars. This loss is greatly reduced in the guide-blade screw-propeller, and is entirely avoided in the turbine-propeller. The third cause of loss, that is, the waste of energy of the feed-water, is experienced only by the jet-propeller, as it has been previously used, and it is from this cause principally that its inefficiency results.

In the boat under consideration, however, careful provision has been made to utilize as much as possible the velocity of the feed-water; and it is in this respect chiefly that she differs from the "Waterwitch" and from the Swedish boat, where nearly the whole of that velocity was lost.

In the Thornycroft hydraulic boat (Plate 1), there is a sudden break in the bottom, just forward of the pump. The bottom at that point has been formed into a great scoop (Fig. 3), rising by a gentle incline to the inlet of the pump, which is placed at an angle, so as to reduce the change of direction imparted to the entering water as much as possible. The velocity of the water causes it to rise in the scoop, and the vanes of the pump are adjusted to pick up the water without shock and gradually to accelerate it to the speed of discharge. Having now put the full energy into the water in the form of velocity, it is simpler to keep it as such, and to get rid of the water overboard as quickly as possible instead of converting it, by means of a vortex chamber, into pressure, in order to reduce the friction through the pipes, as is generally done when the water is carried any distance. In the Swedish boat (Fig. 2) the outlets are carried to some distance ahead and astern. This was done, the Author believes, to reduce the loss by sudden bends, and to increase the leverage for steering, as valves were put at the ends of the passages which were actuated by the steering wheel, and which diverted the water into an athwart-ship direction. This involved a large increase in the weight of water carried and in the friction.

FIG. 4.



"THORNYCROFT" DISCHARGE.

The nozzles in the Thornycroft boat are 9 inches in diameter, and are formed of pieces of copper pipe bent to a radius of 18 inches. They are shown in Fig. 4. Each of these bent pipes is pivoted in such a manner at the point P that either end can be presented to the hole in the side of the boat. No valve is therefore required. The movement is effected by handles in the conning-tower.

There seems to be a considerable difference of opinion as to the best position for the inlet to the pump. Some say that it is immaterial where the water is taken in; while one distinguished engineer thinks that the inlet should be at the stern. His idea, which at first sight seems reasonable, is, that by taking in at the after end water which is at rest as regards still water, then by a gradually narrowing channel and a gradually imparted acceleration giving it a velocity *v*, equal to the speed of the ship relative to still water, and then turning it round and discharging it at the stern with a velocity *v* relative to the ship, and no velocity relative to still water, there is no slip. The efficiency then comes out to that extremely deceptive figure unity, and he states in a Paper "On the efficiency of jet propellers," which appears in the Transactions of the Institution of Naval Architects, that this is a theoretically perfect propeller.[1] This will be examined more particularly in the Appendix; but looked at in a simple manner, suggested by Mr. Thornycroft, the result seems clear. As the propelling apparatus takes up water at rest at the stern and discharges it at the same place and in the same condition; and as the quantity discharged is equal to the quantity received, it seems evident that the inlet and the outlet might be connected together by a pipe and the same water be passed round and round. The result is plainly—no propelling power.

Another point to be considered was whether the outlets should be above or below water. The amount of water discharged, and the resultant reaction, are absolutely the same from a given orifice and a given head, whether that orifice be above or below water. But work is lost in raising the water above sea-level. Another reason for keeping the orifices below the surface was that it was hoped some advantage might be taken of induced currents set up by the discharged water. Experiments were made upon this point independently by Mr. Thornycroft and by the designer of the Swedish hydraulic boat.

In Fig. 5, A is a tank containing water which flows through the pipe B C and is discharged by the nozzle N below the surface of the water in the tank D. The pipe B C is supported

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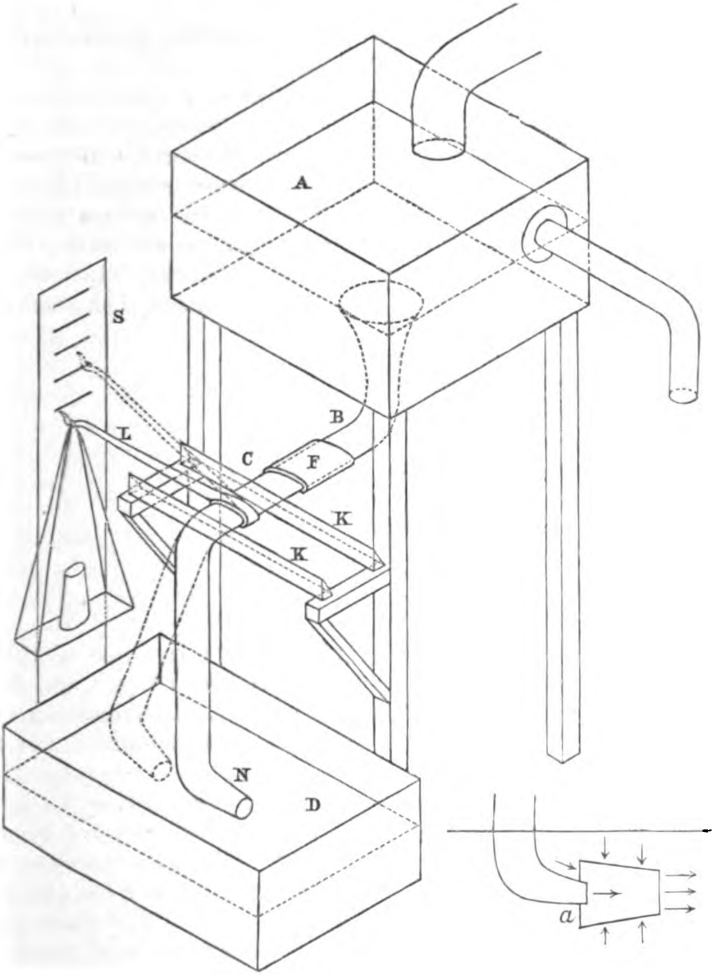
[1] Vol. xii., p. 135

Fig. 5.

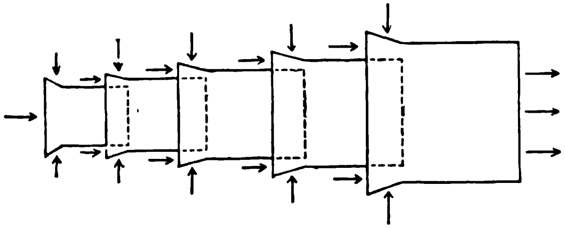


Experimental Apparatus.

upon knife-edges K K. The portion B of this pipe is connected with the part C by a flexible india-rubber pipe F, which permits C to swing back to balance the reaction of the water issuing from the nozzle N. A lever L is attached to the movable pipe C, upon the end of which weights can be hung. The nozzle N being stopped by a cork and the tank A and pipe B C filled with water, the position of the end of the lever L is noted upon the scale S. The nozzle is then uncorked and the water-level kept constant in the tanks A and D. The pipe C swings into the dotted position. Weights are then placed on the lever L until it is brought into the old position, and the moment of the weight balances the reaction of the jet.

If now a hollow truncated cone be placed over the nozzle and the water discharged through it, outside water is drawn through the annular opening *a*, and mixing with the jet has velocity imparted to it. There is a loss of pressure along the inside of the cone and a corresponding increase along the outside which impels it forward, on account of its conical form, and increases the reaction of the apparatus. By placing two or three of these conical tubes one outside the other, as shown in Fig. 6, and thus reducing the speed of the discharged water, and increasing the volume, the reaction of the several cones considerably augments that of the original jet.

Fig. 6.

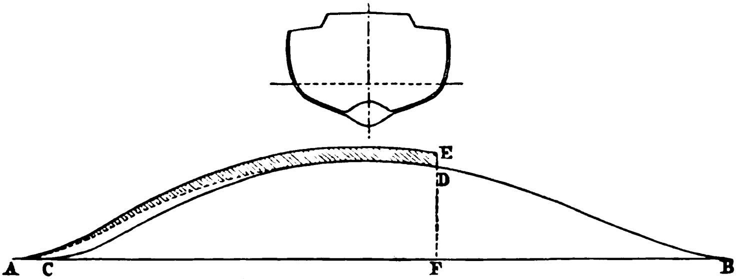


In one experiment five cones were used, and a jet driven by a small propeller was passed through them. They were so arranged that they could be slipped over the jet while the apparatus was working. A weight was lifted by the action of the jet, and the pull of the propeller alone was measured first. The cones were then slipped over it, when the pull was immediately increased in the proportion of 1 to 1½. The experiment gave such unmistakable results that much was hoped from it. When, however, the apparatus was drawn through the water, the friction against the larger surfaces counterbalanced any advantage in reaction, and it had to be abandoned.

Experiments with nozzles towed through the water and discharging below the surface, and then discharging above it, showed very clearly that the resistance of the nozzle far outweighed the loss by raising the water the extra height necessary to keep them above the surface.

Fig. 7 represents the curves of areas of the immersed portions of the hulls of the screw and turbine boats. The length of the former is represented by the base line B C, that of the latter by A B. Ordinates at any point measure the area of the section at that point. The area of the curves thus obtained gives the volume of the displaced water. B is the bow and A and C are the sterns. It will be seen that there is a sudden increase in the sectional area amidships in the hydraulic boat. The height of the ordinate F D represents the area of the section just forward of the inlet to the pump. D E is the area of the inlet. Instead of the area abaft the inlet falling back again, as in all previous hydraulic boats, it is kept at the full amount F D + D E, and continues to the stern at the higher level. An amount of extra displacement, represented by the shaded portion, is thus obtained without the water being conscious of any discontinuity in the form, as the amount which has to be displaced to make room for the increased section is removed by the pump. When the section of the hull is kept the same before and abaft the inlet, fresh water has to flow in from the sides to supply the place of that passing into the pump, and probably disturbs the smooth action of the stream-lines.

Fig. 7.



Curves of Areas and Sections at Inlet.

It has not been found practicable to utilize the full pumping-power of the engines for keeping down leaks. A centrifugal pump will not work unless fully charged; and the supply of water necessary in this case is so enormous in proportion to the size of the boat, that large and complicated valves become needful for closing the inlet and throwing the pump into communication with the bilge, and then to supplement the bilge-water with water from outside as the former becomes insufficient to feed the pump. The difficulty is seen directly when it is stated, that the pump delivers an amount of water equal to the whole displacement of the boat in fifteen seconds. Thus, although not impossible, it would have involved the addition of such a large weight, and the consequent loss of such an amount of efficiency as regards speed, that it was reluctantly abandoned, on the ground that nothing should be allowed to interfere with the highest possible speed being attained.

Although the double-ended form of hull, and the consequent advantage in power of ramming, would no doubt be adopted in an ironclad, it is unnecessary in a torpedo-boat, and some additional speed was anticipated from the retention of the ordinary form.

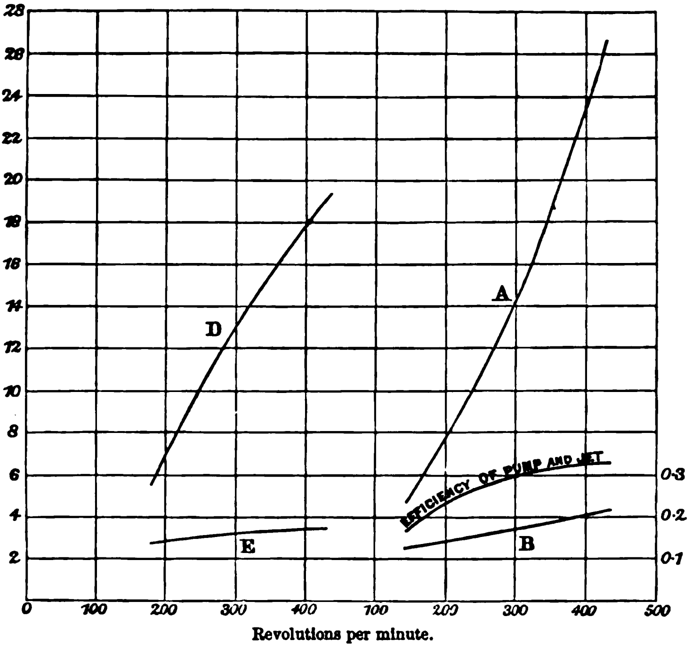
It has always been considered a feature in the hydraulic system that an equal speed is attainable ahead and astern. It is obvious, however, that the form of inlet adopted is unfavourable for going astern. As a general rule, anything designed for locomotion in more than one direction may always be adapted to give a better result when moving in one particular direction than in any other; and when there is an equal power of moving either forwards or backwards, it is possible to effect an improvement in one direction at the expense of the other. Vessels intended to navigate crowded rivers require some power of quickly reversing their motion; but for torpedo-boats, which have to run rapidly up to an enemy and then escape from him, the case is different. The qualities required in torpedo-boats are high speed and quick steering power. Several of the more recent inventions in screw-propellers have had for their object the utilizing of the whole power of the engines for steering; and it is not improbable that, in vessels intended for this service, the complications necessary for reversing the engines may be done away with.

The performance of the boat is shown in Table 2, Appendix II. The quantity of water discharged was about 1 ton per second; the velocity of discharge 37·25 feet per second, and the speed of the boat 21·4 feet per second, or 12·65 knots per hour. The efficiency of the pump and jet combined, *i.e.*, useful work in jet divided by effective HP., was 0·33, and the total efficiency, *i.e.*, useful work in jet divided by indicated HP., 0·25.

The method adopted for measuring the volume of water pumped, and the velocity of discharge, was considerably more accurate than any hitherto employed. In the case of the "Waterwitch," the only measurements taken were with a patent log hung in the jet, and the speed of discharge was not exactly known. The Author has taken the efficiency as calculated by Mr. Brin, which gives the quantity and speed of discharged water shown in Table 2. This velocity, 29 feet per second, approaches very closely to the peripheral velocity of the wheel, 29½ feet per second, and it is probable that the result is somewhat over-estimated.

In the Swedish boat a pressure-gauge was placed in the nozzle, and the calculations were made from the pressures found there.

FIG. 8.



BOAT MOORED. **BOAT IN MOTION.**

In the new boat, a thin plate, $1\frac{5}{16}$ inch square, was attached to the end of a lever, and placed in the jet just where it left the nozzle. The pressure on this plate was recorded by a dynamometer attached to the other end of the lever. The apparatus was so arranged that the pressure could be measured at every point of the jet, and not in the centre of the stream only. The mean pressure in the jet was found to be nine-tenths of the pressure in the centre. From this the velocity was estimated, and from the velocity, the quantity discharged. Fig. 8, curve A, shows the pressure in lbs. upon the $1\frac{5}{16}$-inch plate and lever carrying it, in the centre of the jet, at different revolutions of the pump. Curve B shows the pressure upon the lever carrying the plate, plus a constant weight, and forms the zero. This curve was obtained by finding the pressure upon a similar lever without the plate. The difference between A and B is the actual pressure on the plate. A curve of the efficiency of the pump and jet at different revolutions is also shown.

Curves D and E correspond to A and B, but were measured when the boat was moored. As a test of the accuracy of the method of measurement, a dynamometer was attached to the stern of the boat, and the actual pull found at a given number of revolutions. At two hundred and ninety-five revolutions, the reaction estimated from the curve was 9½ cwt. The pull recorded by the dynamometer was 9⅞ cwt. This was considered very satisfactory, and shows that the efficiency is, at any rate, not over-estimated.

It is possible from the two curves A and D to estimate the effect of the form of inlet upon the efficiency of the jet.

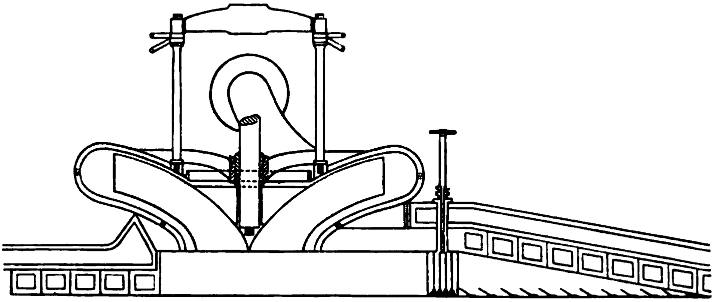
The indicated HP. for a given number of revolutions was found to be the same in both cases. At four hundred and twenty-eight revolutions, the mean pressure when moving was considerably more than the pressure at the same number of revolutions and indicated HP. when moored. This excess of pressure was due to the energy of the feed-water, and corresponded to a velocity of 20·6 feet per second. This shows that only 0·8 foot per second of the full velocity of the boat was lost.

The efficiency of the jet thus appears to be 0·71, and of the pump, 0·46. The calculations are given in Appendix I. In the case of the "Waterwitch," where the whole of the velocity of the feed was lost, the efficiency of the jet was 0·5, and of the pump, 0·47. In the Swedish boat, under similar conditions, the jet gives 50 per cent., and the pump 55 per cent. In this boat an alteration was afterwards made in the inlet, as shown in black lines in Fig. 1, and the speed was thereby raised from 7·87 knots to 8·12 knots.

The measurements were all taken, and the efficiency calculated under the original conditions when the speed was 7·87 knots. The actual HP. expended in driving the vessel at this speed was $78 \times 0{\cdot}214 = 16{\cdot}7$. Suppose this to be increased as the cube of the speed, the actual HP. for 8·12 knots would then be 18·4. This raises the total efficiency to $(18{\cdot}4 \div 78) = 0{\cdot}236$.

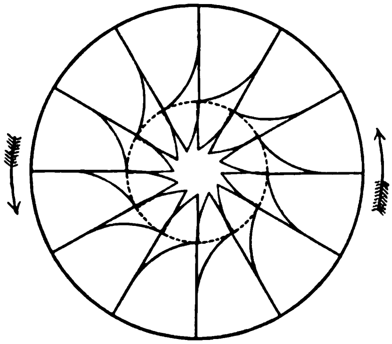
It can be deduced from this that the speed of discharge was raised from 28 to 29 feet per second, corresponding to an increase of head of nearly 1 foot. The velocity of the stream entering the turbine would appear to be only 7·48 feet per second, or a little more than one-half of the speed of the boat. The efficiency of the jet is increased from 0·5 to 0·567. Figs. 10, 11 and 12 show the three forms of centrifugal pump, all reduced to the same diameter.

FIG. 9.



INLET OF "WATERWITCH."

FIG. 10.



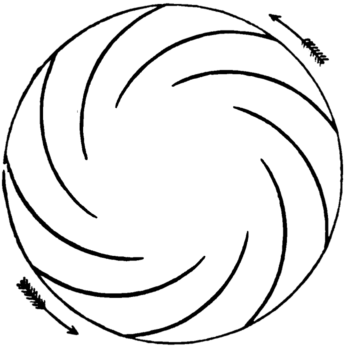
Peripheral velocity 29·6 feet per second.

"WATERWITCH" PUMP.

It should be remarked that the comparison between the performances of the "Waterwitch" and of the two smaller boats, on the basis of the displacement formula given in Table 1, is not a fair one. The coefficient is taken at the maximum speed in each case, and the "Waterwitch" gives a very good coefficient. But 9·3 knots per hour is an easy speed for a vessel of her length, while 12·6 knots is a very difficult one for a boat only 66 feet long.

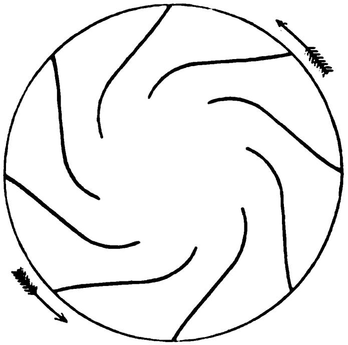
It is alike unfair to compare the coefficients of the two Thornycroft boats at 12·6 and at 17·3 knots.

FIG. 11.



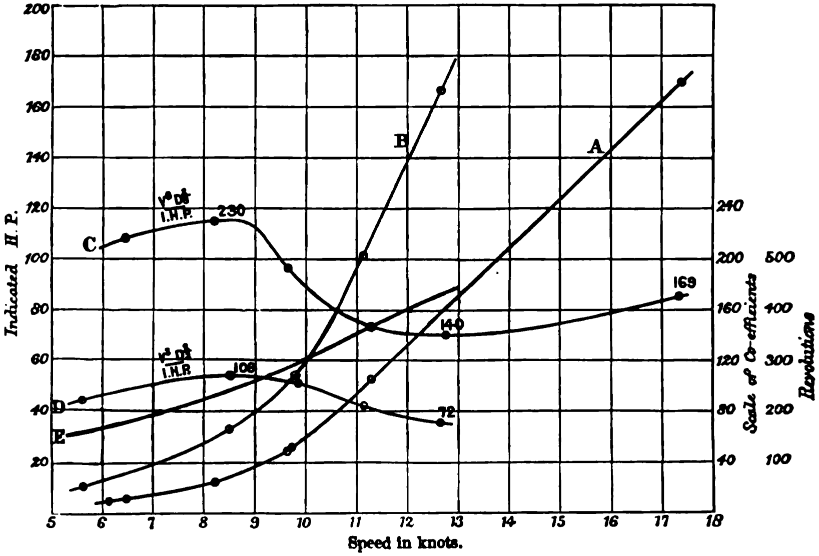
Peripheral velocity 39·36 feet per second.
"SWEDISH" PUMP.

FIG. 12.



Peripheral velocity 56·0 feet per second.
"THORNYCROFT" PUMP.

FIG. 13.



E. Curve of revolutions, and speed of Hydraulic boat.
CURVES OF HORSE-POWER AND SPEED.

Fig. 13 shows the HP. required for different speeds, and the value of the coefficient $\frac{V^3 D^{\frac{2}{3}}}{\text{I.HP.}}$. A is the curve of power and speed of the screw-boat; B, of the hydraulic-boat; C and D show the respective coefficients at different speeds. It will be seen that at 12½ knots the coefficient is at its worst, and that afterwards it steadily improves. The comparison between these two should be taken at 12·6 knots for both boats. The hydraulic is then 72 and the screw 140. This ratio is almost the same as the ratio of the efficiency, 0·254 to 0·5. The maximum performance of the Thornycroft was 108; that of the "Waterwitch," 116. It will be asked, how is it that, although the efficiency of the new hydraulic machine is higher than that of the "Waterwitch," yet the latter gives a better coefficient even at corresponding speeds? Looking at the curve of coefficients Fig. 13, it will be seen that the maximum occurs at 8½ knots. This corresponds to two hundred and thirty-five revolutions of the pump. In Fig. 8 it is shown that the efficiency of the pump and jet is only 0·26 at two hundred and thirty-five revolutions. If the pump had been designed to run at its maximum efficiency at the speed which best suited the boat, a much better coefficient would have been obtained. The indicated HP. at 8½ knots was 33. This multiplied by the maximum efficiency of pump and jet, 0·33, and divided by the efficiency at 8½ knots 0·26, gives 42 indicated HP.; this would admit a speed of 9·2 knots. This speed of 9·2 knots, which would have been obtained with 33 indicated HP. if the efficiency of the hydraulic machine had been 0·33 instead of 0·26, gives a coefficient of 140 against 116 in the "Waterwitch."

In conclusion, it is worthy of note that one of the greatest obstacles to the success of the jet-propeller, namely, the loss of energy of the feed-water, has been overcome. It was clearly foreseen by Mr. Thornycroft, and by adapting the bottom of his boat to meet it in the manner described, he has raised the efficiency of the jet from 0·50 to 0·71.

Unfortunately this obstacle did not stand alone. What efficiency it is possible to get with a centrifugal pump delivering a ton of water per second, with a lift of 21½ feet and of limited weight and dimensions, the Author cannot say. Forty-six per cent. seems very low. Had it reached 70 per cent. the total efficiency would have been 0·385, and the speed upwards of 15 knots. Perhaps this amount of success may yet be achieved for the hydraulic propeller, but it is not likely to be exceeded.

The case at present stands somewhat thus:—

In the screw-boat the efficiencies are—Engine, 0·77; screw propeller, 0·65: total, 0·5.

In the hydraulic-boat—Engine, 0·77; jet propeller, 0·71; pump 0·46: total, 0·254.

The jet as a propeller may be taken as a little better than a screw, but the loss in the pump is a dead loss, and represents about half of the power. In other words, before a hydraulic-propelled boat can be made to compare favourably with one driven by a screw, the pump producing the jet must work without loss.

The Paper is accompanied by numerous diagrams from which Plate 1 and the woodcuts in the text have been engraved.

[APPENDIX.

APPENDIX I.

CALCULATIONS.

The formulas used in the following calculations are given by Professor Rankine in a paper "On the theoretical limit of the efficiency of propellers." [1]

The speed of the water entering the propeller, or the velocity of feed, is taken as simply equal, and opposite to the speed of the ship.

Let this velocity of feed be denoted by v; let s be the true slip or acceleration or additional velocity impressed by the propelling apparatus; then $v + s =$ the speed of discharge relatively to the vessel. If W is the weight of the mass of water acted on in each second, g the acceleration produced by gravity in each second, then the reaction of the water, independent of the form of propelling apparatus altogether, will be $R = \frac{W\,s}{g}$, and the useful work per second done in driving the ship will be $R\,v = \frac{W\,v\,s}{g}$.

If the propelling instrument is so constructed as to act on each particle of water at first with a velocity of feed v, and gradually and uniformly accelerate it up to the velocity of discharge $v+s$, then the loss of work is the least possible. In this case the mean velocity with which the propelling apparatus has to work against the reaction R, is $v + \frac{s}{2}$, and the total work per second is $R\left(v + \frac{s}{2}\right) = \frac{W\,v\,s}{g} + \frac{W\,s^2}{2g}$.

The first term in this equation is the useful work, and the second term expresses the lost work, reduced to a minimum.

This lost work is the actual energy of the discharged water moving astern with a velocity s relatively to still water; and this quantity of energy must be lost under all circumstances. The corresponding value of the efficiency or ratio of useful work is $\frac{v}{v + \frac{s}{2}}$, and this is the theoretical limit to the efficiency of the propeller.

When the energy of the feed-water is lost, the waste per second may be expressed by $f\,\frac{W\,v^2}{2\,g}$; f being a multiplier whose value may range from an insensibly small fraction to unity, according to the degree of suddenness with which the velocity of feed is checked.

In the particular case where all the velocity of feed is lost (so that $f = 1$), the total work per second is

$$\frac{W\,v\,s}{g} + \frac{W\,s^2}{2\,g} + \frac{W\,v^2}{2\,g} = \frac{W\,(v + s)^2}{2\,g},$$

---

[1] The "Engineer," 1867, vol. xxiii., p. 25; and "Miscellaneous Scientific Papers," edited by W. J. Millar, p. 544.

being the actual energy corresponding to the velocity of discharge. The corresponding efficiency is $\frac{2\,v\,s}{(v+s)^2}$.

Case of water taken in at the stern.

The reaction R is the resultant of a forward reaction $\frac{W}{g}(v+s)$ exerted by the water when discharged, and a backward reaction $-\frac{W\,v}{g}$ exerted by the water when received. In this case, as before, the resultant reaction is $\frac{W\,s}{g}$. But s the slip is to equal 0, and the reaction therefore has the same value.

"THORNYCROFT" BOAT.

In Fig. 8, curve A, pressure on $1\frac{5}{16}$-inch plate, and lever in centre of jet, is seen to be 26·75 lbs. zero, curve B, 4·35 lbs. Pressure on plate alone, 26·75 − 4·35 = 22·4 lbs., 22·4 × 0·9 = 20·16 lbs. = mean pressure in jet.

The resistance of a square plate towed through the water, or immersed in a jet, is

$$R = \frac{0{\cdot}627 \times (\text{area}) \times \text{heaviness of fluid} \times (\text{velocity})^2}{\text{gravity}}$$

$$\therefore \text{velocity} = \sqrt{\frac{20{\cdot}16 \times 32{\cdot}2 \times 144}{0{\cdot}627 \times (1{\cdot}3125)^2 \times 62{\cdot}4}} = 37{\cdot}25 \text{ feet per second.}$$

		Square foot.
Area of two 9-inch nozzles	. . . .	= 0·884
„ one 3½ „ „	circulating discharge	= 0·067
	Total area of orifices	= 0·951

W = 0·951 × 37·25 × 62·4 = 2,210 lbs. = weight of water discharged per second;

v = 12·65 knots = 21·4 feet per second;

∴ s = 15·85 feet per second.

$$\frac{\text{Work stored up in water}}{\text{Effective HP. of engine}} = \frac{\frac{W\,v\,s}{g} + \frac{W\,s^2}{2\,g}}{\text{I.HP.} \times 550 \times 0{\cdot}77} = 0{\cdot}45$$

= efficiency of pump, supposing jet to have theoretical efficiency,

$$\frac{v}{v + \frac{s}{2}} = 0{\cdot}73 = \text{theoretical efficiency of jet.}$$

$$\frac{\text{Useful work in jet}}{\text{Effective HP. of engine}} = \frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550 \times 0{\cdot}77} = 0{\cdot}33 = \text{efficiency of pump and jet.}$$

$$\text{Total efficiency} = \frac{\text{Useful work in jet}}{\text{Work expended in foot-lbs.}} = \frac{\frac{W\,v\,s}{g}}{167 \times 550} = 0{\cdot}254.$$

Effect of form of inlet upon efficiency of jet.

Fig. 8, curve D, shows the pressure on plate and lever, with no velocity of feed, to be 19 lbs. at 428 revolutions; zero, 3·55 lbs. Actual pressure on plate in centre of jet, 19 − 3·55 = 15·45 lbs. Mean pressure, 15·45 × 0·9 = 13·9 lbs. Corresponding velocity = 31 feet per second.

The velocity of discharge at the same number of revolutions when in motion is 37·25 feet per second.

	Feet.
Head necessary to produce a velocity of 37·25 feet per second	21·54
„ acquired by pump when producing velocity of 31 feet per second	14·92
Head acquired by velocity of feed . . .	6·62

Velocity corresponding to this head = 20·6 feet per second.
Loss of velocity of feed, 21·4 − 20·6 = 0·8 „

$$\text{Value of } f = \frac{0\cdot8}{21\cdot4} = 0\cdot0374.$$

$$\text{Efficiency of jet } \frac{\frac{W\,v\,s}{g}}{\frac{W\,v\,s}{g} + \frac{W\,s^2}{2\,g} + 0\cdot0374\frac{W\,v^2}{2\,g}} = 0\cdot71$$

$$\text{Efficiency of pump } \frac{0\cdot33}{0\cdot71} \;.\;.\;.\;.\;.\;.\;.\; = 0\cdot46$$

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Calculation of reaction when moored.

	lbs.
At 295 revolutions, pressure on plate and lever	12·6
Zero	3·25
Pressure on plate	9·35

9·35 × 0·9 = 4 lbs. mean pressure.

$$v = \sqrt{\frac{8\cdot4 \times 32\cdot2 \times 144}{0\cdot627 \times (1\cdot3125)^2 \times 62\cdot4}} = 24 \text{ feet per second.}$$

0·951 square foot × 24 × 62·4 = 1,426 lbs.

$$R = \frac{1,426}{32\cdot2} \times 24 = 1,062 \text{ lbs.} = 9\tfrac{1}{2} \text{ cwt.}$$

SWEDISH HYDRAULIC BOAT.

W = 0·864 square foot × 28 feet per second × 62·4 = 1,510 lbs. per second:
v = 13·18 feet per second = 7·8 knots per hour;
s = 14·82 feet per second.

$$\text{Efficiency of pump and jet } \frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550 \times 0\cdot77} = 0\cdot277$$

$$\text{Efficiency of jet } \frac{2\,v\,s}{(v+s)^2} \;..\; = 0\cdot50$$

$$\text{Efficiency of pump } \frac{0\cdot277}{0\cdot5} \;..\; = 0\cdot55$$

$$\text{Total efficiency } \frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550} \quad \ldots\ldots = 0{\cdot}214$$

Effect of alteration to inlet upon efficiency of jet.

78 I.HP. × 0·214 = 16·7 actual horse-power at 7·8 knots.

$16{\cdot}7 \times \frac{8{\cdot}12^3}{7{\cdot}8^3} = 18{\cdot}4$ „ „ 8·12 „

$\frac{18{\cdot}4}{78} = 0{\cdot}236$ total efficiency if I.HP. is the same.

Putting $\frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550} = 0{\cdot}236$, $v + s$ must equal 29 feet per second.

	Feet.
Head necessary to produce velocity of 29 feet per second	= 13·05
„ acquired by pump when producing velocity of 28 feet per second	= 12·18
Head acquired by velocity of feed . . .	= 0·87

Velocity corresponding to this head, 7·48 feet per second.
Loss of velocity of feed 13·7 − 7·48 = 6·22 „

$$\text{Value of } f = \frac{6{\cdot}22}{13{\cdot}7} = 0{\cdot}455$$

$$\frac{\frac{W\,v\,s}{g}}{\frac{W\,v\,s}{g} + \frac{W\,s^2}{2\,g} + \frac{0{\cdot}455\,W\,v^2}{2\,g}} = 0{\cdot}567 = \text{efficiency of jet.}$$

H.M.S. "WATERWITCH."

$v + s$ = 29 feet per second.
v = 15·7 „ = 9·3 knots per hour.
s = 13·3.
W = 6·28 square feet × 29 feet per second × 64 = 11,650 lbs. per second.
I.HP. = 760.

$$\text{Efficiency of pump and jet} = \frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550 \times 0{\cdot}77} = 0{\cdot}234$$

$$\text{Efficiency of jet } \frac{2\,v\,s}{(v+s)^2} \quad \ldots\ldots = 0{\cdot}50$$

$$\text{Efficiency of pump } \frac{0{\cdot}234}{0{\cdot}5} \quad \ldots\ldots = 0{\cdot}47$$

$$\text{Total efficiency } \frac{\frac{W\,v\,s}{g}}{\text{I.HP.} \times 550} \quad \ldots\ldots = 0{\cdot}18$$

APPENDIX II.—TABLE 1.

—	Date.	Length.	Beam.	Maximum Draught.	Displacement.	I.HP.	Speed per Hour.	Midship Section.	$\frac{V^3 \times D^{\frac{2}{3}}}{\text{I.HP.}}$	Number of Propellers.	Revolutions per Minute.
		Ft. Ins.	Ft. Ins.	Ft. Ins.	Tons.		Knots.	Square feet			
H.M.S. "Viper"	1867	162 0	32 0	11 10	1,180·00	696	9·58	337·0	141·4	2 screws	110
H.M.S. "Waterwitch" .	1867	162 0	32 0	11 2	1,161·00	760	9·30	336·0	116·9	1 turbine	40
Swedish screw	1878	58 0	10 9	4 3	20·00	90	10·00	25·0	82·0	2 screws	250
" hydraulic . .	1878	58 0	10 9	4 2	21·00	78	8·12	25·0	52·5	2 turbines	384
Thornycroft screw . .	1883	63 0	7 6	3 8½	12·89	170	17·30	11·9	169·0	1 screw	636
" hydraulic .	1883	66 4	7 6	2 6	14·40	167	12·60	13·4	72·0	1 turbine	428

TABLE 2.

—	Diameter of Turbine.	Area of Inlet.	Combined Area of Discharge.	$\frac{\text{Midship section}}{\text{Area of Discharge}}$.	Velocity of Discharge.	Water Discharged.	Efficiency of Pumps and Jet.	Total Efficiency.
	Feet Ins.	Square feet.	Square feet.		Feet per sec.	Lbs. per sec.		
H.M.S. "Waterwitch"	14 0	28·25	6·280	53·5	29·0	11,650	0·234	0·180
"Swedish" hydraulic	1 11½	1·62	0·864	30·5	28·0	1,510	0·277	0·214
"Thornycroft" hydraulic	2 6	1·52	0·951	14·1	37·2	2,210	0·330	0·254

Discussion.

Sir J. W. Bazalgette.

Sir J. W. BAZALGETTE, C.B., President, said, in proposing a vote of thanks to Mr. Barnaby for his interesting Paper, that it was just thirty years since the subject was discussed at the Institution.[1] The record of that discussion was very meagre, and perhaps it was well that it was so, for report stated that the discussion was exceedingly animated, and somewhat personal, and that the language used on that occasion was not altogether parliamentary. He was sure that on the present occasion the discussion would be animated; he was equally sure that it would be conducted without any personal feeling, in a perfectly "parliamentary" manner; and he could also promise that a true and complete record of the discussion would be given.

Mr. Barnaby.

Mr. SYDNEY W. BARNABY asked permission to call attention to a diagram which had not been alluded to in the Paper, but was illustrated by a model on the table. It represented a small steam launch driven by what he had called a screw-turbine propeller (Plate 1). It had been already described at the Institution[2] and elsewhere, and he only alluded to it because it was really the outcome of the hydraulic-boat. It had been pointed out in the Paper that there were four features in the Ruthven form of pump which militated against its success as a propeller. In the first place, there was the difficulty of getting the water through the bottom of the boat and into the pump without checking the velocity it already had relative to the vessel. In the second place, there was the disadvantage of the extra weight of machinery involved by the necessity of carrying all the water acted upon. Thirdly and fourthly, there was the loss by friction and by bends in the passages. In working out the hydraulic-boat, it occurred to Mr. Thornycroft that if the turbine could be put outside the boat and under the bottom, the first two causes of loss would be avoided. It was also clear that if the water could be made to flow axially through the turbine instead of centrifugally, as in the Ruthven pump, no pipes would be needed, and there would be no loss by friction or by bends. This was what had really been done in the boat, illustrated

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[1] Minutes of Proceedings, vol. xiii., p. 370.

[2] The Author refers to a Paper, "Light-draught Launch," by E. W. Cowan and J. Fawcus, Studs. Inst. C.E., read on the 15th of February at a Supplemental Meeting of the Students, and which will be printed in a subsequent volume of the Minutes of Proceedings.—SEC. INST. C.E.

Mr. Barnaby.

by the diagram and model, to which he had referred. It might be said that it was not a turbine, but it would certainly be an abuse of language to call it a screw. He thought it might be fairly described as a screw-turbine propeller. Another point in its favour was that at least as light a draught (and in some cases a lighter) could be obtained as with an actual centrifugal pump inside the boat. In the second class torpedo-boat, if a screw-turbine propeller had been used, its diameter would have required to have been 21 inches only, and the draught could have been reduced from 2 feet 6 inches, which it was when fitted with the turbine, to 2 feet 2 inches. Therefore, the principal point which Lord Dufferin's Committee had mentioned as a reason for a further trial of the turbine propeller had been secured by a simpler method avoiding a great many of its disadvantages. The steering power also was very superior with the turbine underneath the bottom and near the end to what it was with the nozzles in the ordinary hydraulic-boat, the reason being that almost the full power of the engines was used in both cases; but in the steam-launch (Plate 1) it acted upon a leverage of half the length of the boat instead of a leverage of half the breadth, as in the case of the hydraulic boat. He thought it might therefore be taken that that was at any rate one solution of the problem of hydraulic propulsion.

Mr. M. W. Ruthven.

Mr. M. W. Ruthven said that the Author had taken some trouble to show that great losses were sustained by the hydraulic mode of propulsion; the question, however, was not how much loss was due to the hydraulic method, but was the loss less or greater than by the screw? The safest way to get at that was by a comparison of the results. It was for this purpose that the trials of the "Waterwitch," "Vixen," and "Viper" had been undertaken, though a comparison might also be made with the screw in other

"Viper" Class.		"Waterwitch."		"Philomel" Class.	
9th May, 1866	274·8	Coefficient from	$\frac{v^3 \times M S}{I.HP.}$	14th Mar., 1868	302·0
18th ,, ,,	297·2			8th May ,,	316·0
31st ,, ,,	328·0	1st Jan., 1867	342·7	20th Mar. ,,	299·0
5th Nov. ,,	317·7	17th ,, ,,	327·7	16th April ,,	274·0
4th June ,,	321·0	9th Aug. ,,	351·9	14th July ,,	329·0
5th ,, ,,	335·9	28th ,, ,,	328·7	20th Mar. ,,	279·0
2nd Aug., 1867	379·9	3rd Sept. ,,	303·3	20th May ,,	311·0
5th ,, ,,	437·6	12th Oct. ,,	357·8	19th Nov. ,,	294·0
11th Oct. ,,	424·8			12th May ,,	311·0
			6)2,012·1	27th Oct. ,,	272·0
	9)3,116·9		335·3		10)2,987·0
	346·3				298·7
	3·2		12·2		

Mr. Ruthven. ships. The Author had chosen to compare the "Waterwitch" only with the "Viper," and with a particular trial; but as the results of the trials of the "Viper" and "Vixen" varied nearly 60 per cent. the only approach to accuracy would be the mean of all the trials of both ships, compared with the mean of all the trials of the "Waterwitch." When that was done it would be found that the efficiency of the hydraulic propeller was within 3 per cent. of the screw propeller in the "Viper" and "Vixen"; but as the results of the trials of those vessels varied so much they did not form a satisfactory comparison. There was, however, a class of vessels called the "Philomel" class, with similar engine and boiler-power to that of the "Viper" and "Vixen," whose performance varied less. Now comparing the coefficients of this superior class of ships with the coefficients of the "Waterwitch," it would be found that the hydraulic-propeller in the "Waterwitch" was more than 12 per cent. better than the screw-propeller in the "Philomel" class. In Appendix II., Table 1, the coefficient of the hydraulic-propeller in the "Waterwitch" was put as 116·9, while that of the Swedish boat was 52·5, showing that the former was 122 per cent. better. The "Thornycroft" was stated to be 72, giving the hydraulic-propeller in the "Waterwitch" a superiority of 62 per cent. In Table II., however, this seemed reversed, for the total efficiency of the hydraulic-propeller in the "Waterwitch" was put as 0·180, in the Swedish at 0·214, and in the "Thornycroft" boat at 0·250. Now the use of the column of coefficients was to afford a comparison of efficiency, and this being so, that column in Table 2, showing a total efficiency, must keep the same relations as in Table 1. Mr. Froude, he believed, found that a good screw had a total efficiency of 0·40. Taking this to be correct, and seeing that the hydraulic-propeller in the "Waterwitch" was superior to the screw by 12 per cent., its total efficiency was thus proved to be 0·45; and unless the Author could show that the total efficiency of the screw-propeller was less than 0·18, he must be wrong in putting the total efficiency of the hydraulic-propeller in the "Waterwitch" at that figure.

Captain Heathorn. Captain **Heathorn** observed that five years ago Mr. Ruthven had given him copies of the Tables of the various trials to which he had referred, and he had himself given copies to many members of the Institution. He only hoped that every possible experiment would be tried to see if greater velocities could not be obtained. Mr. Thornycroft had invented something like a Giffard's injector, and had obtained a good result. The force was a flow of water instead of steam; surface friction was at work and produced the

Captain Heathorn.

required result. About eleven-months before his recognition of surface friction as a means of propulsive effort, Captain Heathorn had tried experiments with many kinds of orifices, and found that the force of a stream of water, from a pipe under water, in the still water surrounding was, when once the flow began, not so much dependent on its sectional area, as on the surface contacts; and that if the area of contact was larger in proportion to the area of the section of the flow from a round pipe in one case and one of another form in the other, greater surfaces set up greater friction, greater speed resulted and less water was used. The Admiralty had put him in communication with the late Mr. Froude, who, in answer to a letter of the 20th of March, 1878, said that the proposition which he had laid down as the basis of his view was one with which Mr. Froude was familiar, and he went on to ask in what way Captain Heathorn proposed to apply it to the propulsion of ships. His answer was:—Contract the orifices or jets of H.M.S. "Waterwitch" to a certain form and do the same to the exit ends of the screw-channels of H.M.S. "Bruiser." Screw-channels, he believed, were invented by the late Mr. Griffith. Now that the Admiralty had a boat he hoped that they would persevere and find out what hydraulic power really was.

Admiral Selwyn.

Admiral SELWYN considered the Paper an important contribution to the history of the subject. If any means could be adopted, even at the sacrifice of speed, to prevent ships and their crews going suddenly to the bottom by the impact of a torpedo or a ram, he was sure Englishmen would advocate spending a little more money for the extra power required to give the necessary velocity, and at the same time secure the other advantages. He doubted the accuracy of the statement in the Paper that the principles underlying the subject were perfectly understood; he thought that, otherwise, none of the experiments would have been made on the basis on which they had been carried out in the "Waterwitch" and the other similar vessels. The hydraulic arrangement seemed to present advantages which were apart from its use as a propeller; but wherever it was necessary to put a scoop into the bottom of a ship it would be found that there was a strong objection to it, because the scoop would be the first thing to encounter the ground on getting into shallow water. It would not, therefore, be liked for that purpose, however applicable it might be to a torpedo-boat. He had lost all faith in coefficients. He had lately seen a statement that the large and fast steamer, the "Phaeton," sister ship to the "Iris," had had about 600,000 lbs. weight of engine taken out, the engine-power being reduced from 7,000 HP. to 5,000, Admiral Selwyn. and had been driven faster (more than 18 knots, with the same displacement and the same shape), and he confessed that coefficients would require a little more explanation before he could recognise their applicability to the subject. Engineers were apt to make a formula to fit certain cases, and to lay it down that those cases were the rule, and that all abnormal cases, as they were called, were the exception. It was strange that with the addition of 600,000 lbs. to the armour-plated deck, one vessel with 5,000 HP. went 18·3 knots an hour, while the other vessel with 7,000 HP. went 18 knots. An experiment mentioned to him some years ago by Mr. Gwynne, of Hammersmith, gave the following results: Diameter of disk, 30½ inches; orifice, 8 inches; revolutions, eight hundred per minute; indicated HP., 100; head attained, 90 feet, with a discharge of 800 gallons per minute; thus there was a pressure of 45 lbs. per square inch. The effective thrust was 2 tons. Of course to the 45 lbs. per square inch would have to be added the 800 gallons per minute discharged at the top of the 90 feet. There was an important anomaly in all the records of the "Waterwitch." Engineers would readily admit that there was no departing from the law of action and reaction being equal and contrary, and when any one told him that a pump, whose peripheral velocity was 29·3 feet per second, gave a velocity of discharge of 29 feet per second, he thought some other explanation should be sought than that which was found in the pump itself. Unless the velocity of the feed was there utilized, he did not see how such an effect could be produced. If it were produced, the pump would be undoubtedly an absolutely efficient machine. It might be expected that as the disk-pumps were enlarged there would be an increase of efficiency. All experience pointed to that. While in the smaller pumps the velocity of the issuing water was not more than about 50 per cent. of the peripheral velocity, in the larger pumps 60 and even 70 per cent. had been obtained. Distrusting formulas, he took the percentage by calculating the revolutions into the circumference, finding the peripheral velocity of the disk from that, and getting the discharge as a percentage of that peripheral velocity. When 37·2 feet per second had been obtained as the velocity of discharge, and the velocity of feed was 20 feet per second, he presumed that only 17·2 feet per second had been communicated by the engine to the water. If that was the case, might not the anomaly in the "Waterwitch" be attributed to the velocity of feed from those inlets? A set of mouths, immediately under the pump scooped downwards, as was the case in the later trials of the "Waterwitch," would give a certain velocity of feed into the pump, and

Admir Selwy

that might be added to the peripheral velocity, so explaining what otherwise seemed to be utterly inexplicable? He had found that various sources were accessible for an account of the revolutions of the "Waterwitch" engines, and while he had found them sometimes stated at forty-two, they occasionally descended to forty per minute. Engineers, he thought, would be thoroughly aware of the vice of taking revolutions either through another observer or at a different time from that at which the indicator diagrams were recorded. Any possible HP. might be indicated in such a way, and give rise to a very false basis of coefficients. It appeared that in the "Thornycroft" propeller a velocity of 56 feet per second had been obtained—he presumed a peripheral velocity—and the velocity of discharge was 37·25 feet. That velocity was much more important than quantity of discharge. The volume did not matter in the least in regard to the velocity obtained. The whole power of the engines might be thrown away in lifting a mass of water, but if the necessary velocity to produce the speed sought was not communicated to the water, most engineers would say it was vain to seek it at all. It would be interesting to try an experiment first by towing the boat with her inlet so arranged and the scoop so arranged at the velocity which she had obtained with the engines at rest. It would then be seen how much the velocity of the inlet-water was diminished by passing through the pump at rest. Next, to tow the boat with such revolutions of her engines as would give a peripheral velocity equal to the speed of the boat. And, lastly, at the calculated velocity of feed. Those three experiments would bring out exactly the efficiency produced by the feed as distinguished from that produced by the pump. Now, supposing that the revolutions of the screw-engines had not been changed, but that the diameter of the nozzles had been diminished from 9 inches to 6 inches, and the disk increased to 2 feet 9 inches, what might be expected? He thought it would be found, taking the basis obtained by the Author's experiments of 56 feet peripheral velocity, and 37 feet velocity of discharge, that 60 feet per second velocity of discharge should be obtained with 91 feet per second velocity of periphery. That would correspond, provided the water thrown was sufficient, to bring out a proportionate pressure. He was not aware whether the pressure per square foot of midship-section to drive the boat 17·3 knots per hour had been calculated, but, whatever it was, it could no doubt be made up. The velocity of 91 feet per second peripheral, and 60 feet discharge, would give about 17·7 knots per hour. He was sure that would be impracticable with a less speed of discharge-

Admiral Selwyn. water. If the number of revolutions per minute were diminished, the speed of the boat could be arranged without any possibility of change. Every experiment he had made brought out the fact that the speed of revolutions was indissolubly connected with the speed of the boat. If, therefore, the principles underlying the subject had been thoroughly understood, the first question asked would have been, "What speed is it wished to get from the boat?" That would govern the revolutions or the revolutions multiplied into the periphery of the disk. The disk might be made a little larger and then the revolutions might be diminished, or the revolutions might be increased and the disk diminished. But, as Mr. Gwynne's experiments showed eight hundred revolutions producing a thoroughly good effect, with one 8-inch issue, there need be no fear. He had himself driven a small 1-foot pump at one thousand revolutions, and on the basis to which he had referred he had obtained exactly the calculated speed of the boat. The pressure per square foot midship-section was, as nearly as he could make out at the velocity and size of nozzle stated, 120 lbs. He presumed that that would be scarcely sufficient for a speed of 17·7 knots per hour, but it could be easily increased. In regard to the possibility of pumping the ship out with the full power of the engines, if any issue was opened from a drowned compartment (where the water must be on a higher level) to the turbine, he failed to see why the turbine should chose between the bilge-water and the other water. He thought it would certainly take a larger quantity from the easier path of the inlet, but that it would take a portion from the bilge that would probably be sufficient to keep the boat from sinking, or result in so far prolonging its floating above water as to save human life. That result would be sufficient to justify any trials. It could scarcely be supposed that in the event of another war there would not be a lesson to learn as to the effects of rams and torpedoes. Mr. Thornycroft had done so much and so well in providing the means of destroying the British and other navies that it was only fair to ask him to throw a little of his energy, skill, and genius into the question of how to save human life and the expenditure of ships.

Sir Edward Reed. Sir EDWARD J. REED, K.C.B., M.P., thought the Paper was a very important contribution to the knowledge of the subject of hydraulic propulsion. The last bearing on the subject was the comparison referred to by Mr. Ruthven and other speakers between the "Viper" and the "Waterwitch." Admiral Selwyn had expressed his distrust of coefficients and of formulas, and Sir Edward Reed supposed that all engineers distrusted them when they only took

Sir Edward Reed.

account of a fractional part of the circumstances they wished to consider. There were very peculiar circumstances in the case of the "Viper" and "Waterwitch" trials. In the first place he might say, as the designer of the boat, that the "Viper" was about the very worst screw-propelled vessel that had ever been built, and that for a very good and sufficient reason. She had to be designed to pass through certain locks; her length and her breadth therefore were arbitrarily determined, and her depth also had been similarly determined. Having thus a parallelopipedon which was to embrace the "Viper," there had to be carried by it certain armour and guns, and particularly as much as possible of the armour. The speed was of very minor importance, and it had been deliberately subordinated to the armour-carrying and gun-carrying power. It would therefore be seen that the "Viper" and "Vixen" were rectangular boxes, slightly improved, with small regard to propulsive advantages, and he thought they were about as bad vessels as could have been designed. Admiral Sir George Elliot, impressed, like Admiral Selwyn and many others, with the great advantages of hydraulic propulsion to Her Majesty's ships at sea, put a strong pressure upon the Admiralty to get the hydraulic-propeller tried, and holding at the time a high official position, and being an officer of great experience at sea, and of much deserved influence, he succeeded in inducing the authorities to try a hydraulic-propeller in a vessel of the class referred to, or a vessel of about the same dimensions. There were reasons, however, tending to show (the figures given by the Author did not fully demonstrate it, but he had no doubt Mr. Ruthven would readily concur) that the form of the "Waterwitch" was superior to that of the "Viper" and the "Vixen," because when she was designed some limitations imposed upon the other vessels had been removed. A considerable improvement in form was realized, and the trials were made, and, as far as his memory went, the figures given by the Author fairly represented the state of the case. Taking Mr. Ruthven's figures, it would be seen that the bad screw-propelled vessel was a little better than the hydraulic-propelled vessel, even at the low speed of 9½ knots per hour. The trial certainly made a good show for the hydraulic-propeller by comparison with a bad screw-vessel—a better result than had been expected by many persons; but what would happen when higher speeds were attempted? The Author's Paper contained a case in point. The Government had sought to give effect to the recommendation of some of the members of Lord Dufferin's Committee, or of the Committee itself. They had gone to a gentleman who had dis-

Sir Edward Reed. tinguished himself in the design and production of vessels of the torpedo class, Mr. Thornycroft, to whose genius the introduction of these light and extremely fast vessels was due, and they had asked him to do the best he could in establishing a comparison between the hydraulic and the screw-propeller designed for high speed. The result was that with a given HP. in two vessels made as nearly alike as the circumstances would admit, the hydraulic vessel steamed at a little over 12½ knots per hour, and the screw-propelled vessel at a little over 17¼ knots. Admiral Selwyn had referred to the statement in the Paper that the jet as a propeller might be taken as a little better than a screw, but he had forgotten to complete the sentence: "But the loss in the pump is a dead loss, and represents about half of the power." He did not know what the experience of others might be, but he could say for himself that, if he had to devise a fast or any other steam-vessel, to lose half the power in order to secure an efficient propeller would be a very sad imposition to have placed upon him. He was unable, as he always had been, to anticipate any satisfactory result from the hydraulic system of propulsion as applied by means of internal pumps in the ship. He sympathised with the objects that Admiral Selwyn and other naval officers had in view, and he should be delighted to see growing out of the discussion some relief from the loss which the system always involved when so applied. He confessed that his sympathies went very strongly in favour of the apparatus described by the Author after the reading of his Paper, viz., the application outside the vessel of an arrangement by which the good effects of hydraulic propulsion might be obtained in a large degree without its serious disadvantages. With regard to Mr. Ruthven's reference to the mode of conducting the trials, he thought he ought in fairness to say that everything had been done at the time that could in reason be done to satisfy Admiral Elliot, who was known to be more or less in communication with Mr. Ruthven. He had also to state that he believed that the consideration of the subject had been brought to an end because the discussions between Admiral Elliot and the Admiralty had passed away from the efficiency of the jet system to questions about the indications of power in the engines of the "Waterwitch," and other collateral matters, so that the essential part of the subject had in some way fallen out of discussion. He was sure it would be the desire of the Council and of the members that every possible opportunity should be given to gentlemen capable of discussing the subject to direct the minds of the members to some methods, if methods there were, by which to get rid of the enormous loss sustained within a ship by the application of the hydraulic system for its propulsion.

Sir Edw Reed.

Sir Geo Elliot.

Admiral Sir GEORGE ELLIOT, K.C.B., said he would not venture to enter the domain of science; all that he knew of hydraulics he had learned from personal observation of the practical results which had been hitherto obtained. He was greatly pleased when he heard that the subject was to be discussed by the Institution, on account of the great advantages to navigation which would accrue even from a partial application of hydraulic propulsion to ships. He did not think that hydraulic propulsion had ever yet had fair play; it had never yet been tried on its merits. Certain conditions had been attached to the experiments which he thought had marred the best efforts of engineers. The "Nautilus" had not been designed with the sole view of developing hydraulic motive power in a ship; the owner had a commercial object in view. He looked to a commercial enterprise, inasmuch as the vessel was designed to run up the Thames to Richmond; consequently, with a light draught and having to carry a large number of passengers, she had full lines; still the speed realized by that vessel was 9½ knots per hour. He thought the Author had made a mistake when he said 8·32 knots. She was raced on the river against a vessel called the "Volunteer" down to Gravesend, and she distanced her. The "Volunteer" was a vessel of 24 HP., whilst the "Nautilus" was 20 HP. She afterwards raced constantly with the "Citizen" boats, and to his knowledge she beat them. Then a challenge was held out to any paddle-boat on the river, but it was not accepted although an offer was made to pay the expenses. No one could say that the "Waterwitch" was a good specimen of naval architecture in which to try an experiment. The constructors had a very difficult task in making armour-clad gun-boats, and the designer looked upon them as very bad specimens of naval architecture. However, the "Waterwitch" in her first trial at the Maplin Sands realized a speed of 10 knots per hour. At that time in a contest between the "Viper" and the "Vixen," her two sister vessels, the latter was the slower of the two, therefore the trials afterwards took place between the "Viper" and the Waterwitch"; the former vessel at the Maplin Sands obtained the same speed as the "Waterwitch." They were afterwards taken to Portsmouth where the trials went on. The builder's trim of the "Viper" was 1 foot at the stern, at which she was run at the Maplin Sands; but at Portsmouth she was brought down to 2 feet at the stern, and the bow lines were thus lengthened, whilst the "Waterwitch" was compelled to remain

Sir George Elliot. on an even keel. The trials gave the advantage to the "Viper" ot $\frac{1}{4}$ of a knot speed. In the "Waterwitch" the water came in to the turbine from forward through a canal, with holes perforated in the bottom. The turbine was raised unnecessarily high, and the discharge pipes were raised still higher, in order to test the differences between discharging the water above water when she was at her light trim, and in the water when she came down to her load draught. He considered both those features to have been adverse to the success of the hydraulic method of propulsion. Those vessels went to sea, but they were found to roll so heavily as to be unseaworthy, and they were afterwards laid up and lightened and taken out to Bermuda; the "Waterwitch" was laid up in ordinary. Some years afterwards, when he was appointed Dockyard Superintendent at Portsmouth, the Admiralty allowed him to have the "Waterwitch" to make some experiments. He had an idea that leading the water in through the bottom of the vessel in a canal, by which a large quantity of water was carried about, necessitating increased displacement, was a mistake, and he was determined to ascertain how far it would be advantageous to bring the water in from forward or from directly underneath the turbine. The trials were carried out very carefully by the dockyard officers. He merely asked them to close the canal and to open underneath the turbine a hole and to insert a tube, and let the water enter directly underneath the turbine. The vessel was taken into dock and cleaned, put to a certain trim, run at the mile, and her speed ascertained. She was then docked. The alteration was made; she was again cleaned, put to the same trim, taken to the mile, and the result was $\frac{1}{2}$ knot additional speed per hour. He then thought that the turbine to be at its best should be brought as low in the vessel as possible, the water admitted from underneath, and the discharge-pipes horizontal. In the "Waterwitch" they were raised, as he had said, which was certainly an objection. He still believed that, in order to try the hydraulic-propeller at its best and give it fair play, a vessel should be designed for the purpose, a double-ended vessel with a rudder at both ends, on an even keel, and the hydraulic-propeller as low as possible, in point of fact to work in the water. He had reasons for believing that bringing the water up to the turbine from directly underneath was an advantage. His idea was that directly the turbine was turned a hole could not be made in the water, and a column of water at a certain speed was immediately set in motion, which was greater than the speed at which the ship was going through the water; consequently the greater velocity overcame the lesser velocity due to the momentum of the vessel; in

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fact there was no loss of power. If the turbine was very low down the bottom of the turbine might almost be brought to the sea itself. Having a rudder at both ends the vessel would go equally fast both ways; in fact, in the trials in the "Waterwitch," although the water was brought in forward it did not make much difference in her speed one way or the other. That told very much against the attempts that had been made to draw the water in from forward by pipes from the bows. He could understand the object in view; and theoretically he could see that it appeared to be a wise one; but as far as that trial in the "Waterwitch" went, it proved not to be so. Then he made an experiment to ascertain what the size of the discharge pipes should be. The Admiralty were again good enough to carry out his experiment, by putting a flap on a hinge inside the nozzle, which could be pushed out, so as to close the aperture or leave it wide open. The speed of the water at the discharge was measured, and it was found when the nozzle was half closed the water was going out at double the speed; therefore the same quantity of water was discharged as when it was wide open—the speed of the vessel in both instances being the same. That showed that the nozzles of the "Waterwitch" were unnecessarily large, because he believed that the best result was obtained when the pipes were completely filled, but trials were made at different revolutions of the turbine, and it did not appear to signify much. He did not agree that the turbine would not work unless it was full. The Author had described all the advantages (and they were very great) of the turbine as a propeller for ocean purposes, but he had not specially named one benefit which he thought the most important. If a vessel could be stopped, without any reference to the engine-room, in her own length, and if two vessels meeting could do the same, he believed there would be very few collisions. Another feature was the advantage that it afforded in foul weather. The accidents in screw-boats that then occurred were generally caused from the racing of the screw; but the greatest amount of motion a ship could be put into would not in the slightest degree bring a greater strain upon the engines of the turbine. After a considerable time, the subject was again mooted at the Admiralty, and then came the trial of the Thornycroft torpedo-boat, respecting which he was greatly pleased to think that the experimental trial had been placed in the hands of that firm, who, he had no doubt, had taken pains to make it a success. But as a competitive trial it was inadequate. The Author had stated that the object of the Admiralty was to have vessels of very light draught of water in

Sir George Elliot. waters so shallow as scarcely to afford sufficient immersion even for twin screws. In a fair trial both vessels should have been put to the same test, which was the draught of water. But in the case of the turbine the draught of water was 2 feet 6 inches, while that of her opponent was 3 feet 8½ inches; and in the screw-boat the shaft was on the keel, and nearly half the blades revolved below the keel, giving a large diameter to the screw, added to the extra draught of water. In this competitive trial the screw torpedo-boat would require a depth of 6 feet of water to work the screw, whilst the turbine boat would only need a depth of 2 feet 6 inches to move about in. That was unfair to the turbine which had not those advantages. The two trial vessels should have had the same draught of water, similar displacement, and similar power. If the draught had been 2 feet 6 inches, and the screw had been limited to that diameter, he thought that a very different result would have been obtained. He should be much pleased if Messrs. Thornycroft and Co. would build a boat exactly as he had described, with her turbine low down, with the entrance of water from below, with the discharge-pipes horizontal ejecting the water under water, and with the valve like that in the "Waterwitch," and then see what speed they could get out of the screw with a diameter of 2 feet 6 inches with the same power. Then again one boat was brought by the stern, and the other was on an even keel; but both boats ought to be confined to a given draught of water. He would give the turbine boat rather a full body, so as to lower the turbine and get as much benefit from the full diameter as possible. In the vessel in question, the diameter of the turbine was small, and the discharge-pipes were long. It appeared to him that the importance of centrifugal force had been lost sight of, and this he had always understood to be an advantage in the turbine, by not having a large diameter so as to discharge the water at a rapid rate through the pipe, and reduce the revolutions of the turbine. Another advantage of admitting the water from below was that it was not necessary to carry the same volume of water in the canal as had to be carried in the "Waterwitch"; it was only necessary to carry the water which the turbine itself contained. When he spoke of the partial success of the turbine, he referred to its use in sea-going vessels as an auxiliary power. With power sufficient to give a speed of 8 knots per hour, to get across calms and in and out of harbour, an enormous advantage would be gained. Unfortunately the hydraulic-propeller had not had many friends. Whenever he had approached the shipbuilder on the subject he had been told that it would cost a great deal of money being a novelty, and that at once had put an end to the idea. On one occasion at Portsmouth a lady, in delicate health and unable to stand the vibration of a screw, was towed about in her yacht by a tug. He endeavoured to get a turbine put into a vessel that the lady was having built, but it was stated that it would have added £6,000 to the cost. He should be glad to see hydraulic propulsion tried still further, and felt sure that better results would be obtained if it were tried fairly on its merits. Sir George Elliot.

Admiral Ryder. Admiral RYDER observed that he had paid some attention to the subject from the commencement. He had taken a run in the "Nautilus," and two or three trips in the "Waterwitch." A few days ago he went out from Portsmouth to Spithead in Torpedo Boat No. 87, which was irreverently called "The Squirt." It was a moderately fine day. The vessel was a remarkably good shape forward for riding over the sea, but the two tubes for ejecting the torpedoes were unfortunately conduits for seas that came over forward, and which dashed through the tubes against any one on deck with great force. The tubes should have wooden plugs. When going at 12 knots an hour, he said to the lieutenant in charge, "I think you may ease her," and his reply was, "We must either stop or go full speed." If that was the fact, it was certainly an unfortunate position for one of Her Majesty's vessels to be in. The sensation was peculiar when the boat stopped short. She stopped very dead, which of course was what was wanted in the case of a torpedo-boat. But he could not make out why the ejection orifices were above water. The noise was like soda-water bottles going off continuously. The tremendous noise would, he should think, on a calm day inform an enemy 10 miles off of the approach of the boat. He could not believe that it was in the "specification" that the orifices should be above water. Perhaps there was some good reason for it, but from his point of view as a sailor this feature was a most objectionable one. The nuisance of the noise was intolerable, and he thought that any Admiral would send the boat home by the first ship; he hoped that in future designs the orifices would be under water. One of the great advantages of that class of torpedo-boat was the freedom from danger of fouling. Seafaring men and engineers could easily picture what would happen on going into a harbour protected by nets with the ordinary form of torpedo-boat having one or two enormous screws; these would be sure to come in contact with any obstruction of that kind. It was all very well for the open sea, but he did not believe that such a boat would get inside an enemy's

Admiral Ryder. harbour at all. The turbine principle was a capital one for that purpose, because there could be no fouling by nets or anything of the kind. Another valuable feature was the power of changing the course to the opposite direction suddenly. A torpedo-boat fitted with any system of screws could work end on, bow foremost, but could do nothing against the enemy stern foremost; whereas if the turbine principle were further developed, and applied to larger vessels, an enormous advantage would be gained; indeed, the utility of the ship might be almost doubled. After having gone through the enemy's fleet, and struck or missed a ship, a turbine vessel might at once reverse her engines and retrace her course, without exposing her side, as the screw-vessel would do when turning. Of course the great question was whether the numerous advantages named by the Author, not omitting that invaluable feature that the turbine-propeller could not be "pitched" out of the water, as the screw frequently was, were worth paying for, and to what amount. They might be paid for in reduced speed. Would it be worth while sacrificing 2 knots, or how many knots, an hour to obtain the advantages that had been described? A committee of naval officers would soon come to a determination on that point. If the authorities would not pay in speed, would they pay in money? It might be necessary to get a larger or a narrower boat, or to give up some other feature to which importance was attached. He agreed with Admiral Sir George Elliot and Admiral Selwyn that the turbine question was not yet settled. The system had been tried by different nations. Swedish naval officers attached great importance to it. When he was commander-in-chief at Portsmouth, whenever a Swedish man-of-war came in, he always asked the officers how the turbine system was getting on. Their reply was that it was so unpopular with the Construction Department that they were not at all sanguine that it would be favourably received. At length, however, a turbine vessel was tried, and on enquiring the result, the reply he received was, "They have managed to make it fail." A round sum in thousands of pounds should be offered for the best design of a turbine torpedo-boat, capable of a speed of 20 knots per hour, and with orifices under water.

Mr. Wright. Mr. J. Wright, C.B., Engineer-in-Chief of the Navy, said that the subject had been so fully and fairly discussed in the Paper that he had very few remarks to make; but there was one point which had not been much referred to during the discussion, namely, the great advantage supposed to accrue to a vessel with a hydraulic-propeller in turning power. It had been tried in the case of the

"Waterwitch" and the "Viper," and the advantage was certainly very much in favour of the "Viper." It took the "Waterwitch" twice the time to turn a circle that the "Viper" took when one screw was worked ahead and the other astern. Another point of importance was the great advantage claimed for the hydraulic-propeller in clearing the ship of water in the event of her getting a hole in the bottom. The Author had referred to the practical difficulties which would have to be overcome in opening and closing large sluice-valves in order to carry out such an arrangement. From some of the preceding remarks, it might be supposed that the naval architects of the present day had given no attention to the important matter of saving life on board ship, in the event of a large hole being made in the bottom; but any one acquainted with modern ships of war would know that an extensive subdivision was carried out for that very purpose, so that if two, or even more, compartments were penetrated, a large power of flotation would still be left. If a hydraulic-propeller were fitted for ejecting water from the ship, it was just possible that if a hole were made by a torpedo in the compartment containing the propeller, the propeller would be disabled. If, again, a hole were made in another part of the ship, there would be a difficulty in getting the water under the propeller in anything like sufficient quantity to keep it charged. No doubt Admiral Sir George Elliot had made an improvement by closing up the holes in the "Waterwitch's" channel forward, and opening a direct hole under the bottom. The coefficients of the performances (though one gentleman had declared his disbelief in coefficients) showed an improvement of something like 15 per cent. But with regard to other experiments made by Admiral Sir George Elliot, in curtailing the size of the discharge-nozzle, there was a decided falling off to the extent of over 7 per cent. There was another point with reference to the "Viper," namely, that the screw-propeller, acted on ten times the amount of water that passed through the turbine. If any one would examine the theory of the subject, he would find that that was a very important matter in regard to the superior efficiency of the screw. Comparing the two torpedo-boats in the same manner, it would be found that the screw-boat acted upon five times the amount of water that the turbine had acted upon. Mr. Wright.

Mr. R. SENNETT observed that there could be no doubt that the torpedo-boat referred to was the outcome of engineering skill of the highest order, and of workmanship of the greatest perfection; and with what result? Practically this, that the speed attained Mr. Sennett.

Mr. Sennett. was no greater than could be obtained from a screw-boat of equal size working at about one-half the power. The causes of this defective performance were not difficult to determine, and were, in the main, inherent to the system. All propellers worked on the same principle, namely, the projection of a volume of water sternward, and the thrust of the propeller was measured by the momentum or change of momentum produced. The more directly sternward the water was projected the greater the efficiency, all transverse motion representing wasted energy. In the jet every particle was sent directly astern, and therefore, theoretically, neglecting friction, the jet was the most perfect of propellers. It was from practical difficulties that the pump failed as a propeller; and though engineers were loth to apply the word impossible, yet in this case many of the obstacles were apparently insurmountable, and in his opinion the pump was never likely to become a general and efficient propelling agent, though it might perhaps be useful in some special cases. This arose mainly, as had just been pointed out, from the large volume of water that had to be dealt with, and the large orifices that would be necessary in the ship's sides to obtain high efficiency of propulsion. The thrust of a propeller depended on the mass of water operated on, and consequently, other things being equal, the propeller that could operate on the greatest volume of water would be the most efficient. Perhaps the custom of speaking of this propeller as a jet-propeller might unintentionally convey the idea that it was different in principle from other propellers. This, however, was not so. The jet was no more the propeller in this case than was the race of the screw or paddle in ordinary ships. As a matter of fact, the pump was the propeller, for it was the agent that drove the water astern from the momentum of which propulsion resulted, just as the screw and the paddle were the machines for driving the water astern in ordinary cases. In the hydraulic-boat the propeller was placed inside; in the paddle and screw outside; the action in each case was the same, only the machine was different. Until pumps were fitted that could cope with as large volumes of water as the feathering-wheel, and at about the same speed, the hydraulic propeller would not be able to approach in practical efficiency either the screw or the paddle, irrespective of the greater losses that might be expected from the friction of the water in the passages. If the pump were made to satisfy these conditions the large orifices necessary would, in most cases, be inadmissible, whether above or below the water. Then also the question would arise, would it be desirable to place such a propeller in what

Mr. Sennett.

would practically be a trunk in the ship, occupying valuable space and involving the carrying of a considerable weight of water? Or would it not be preferable to place the propeller outside the ship, and make proper provision for a sufficient flow of water to it, without compelling the water to pass through and be carried by the ship? He had little doubt as to the answer that would be given by the vast majority of naval architects to this practical question.

Mr. Thornycroft.

Mr. JOHN I. THORNYCROFT said that Captain Heathorn appeared to convey the idea that the friction of the external surface of the stream of issuing water had some advantageous influence in propelling a vessel, and he applied his remark to an experiment which he had made where an induced current of water was used to augment the propelling effect. He desired to go into some detail in order to explain the matter. A stream of water issuing from a vessel had a re-action depending simply on the weight discharged in a given time and the velocity of discharge; the effect was unaltered by the form of aperture at which the stream found exit. The stream being submerged made no change in the re-action of the initial stream, but it then was in a position to induce other streams which might act favourably or unfavourably in propelling—favourably when the pressure was reduced on a surface attached to the vessel which had a motion towards the loss of pressure, and the contrary if motion of the surface was in the opposite direction. One way of taking advantage of this principle was to surround the stream of water from an ordinary screw-propeller by a conical tube of such dimensions as to allow an annular stream to pass through it around the stream from the propeller. When this was so placed that the initial stream entered at the larger end of the portion of a cone, and by the friction of its external surface and intermingling with the surrounding water induced a stream through the coned tube, the pressure within this tube would be reduced and the forward component of the difference of internal and external pressure would be available for propulsion. But in order to estimate the useful effect, the friction of the surface of the coned tube must be taken from the forward component of the pressure already alluded to. Experimentally he found that if a vessel was fitted to utilize the currents so generated, although it would be able to exert a greater pull at a fixed object in still water this advantage would diminish as speed was attained, and it appeared that at a useful velocity of vessel the apparatus was a failure: but he thought the result obtained by Captain Heathorn must be explained in another way. He thought that Captain Heathorn had

Mr. Thornycroft.

fallen into an error. He had described an experiment that he had made, and he understood him to say that from water issuing from a pipe he got a certain re-action, and by contracting the pipe so that less water came out he got a greater re-action. He thought it was evident that in the first case most of the work was done by the friction of the water in the pipe, and in the second case, although less water came out, it came out at a higher velocity, and so the result was obtained. Captain Heathorn's allusion, therefore, to the apparatus to which he had referred was not correct. Admiral Selwyn had alluded to a pump, which he said gave a certain duty, and he spoke of 100 HP. working a disk 30½ inches in diameter at eight hundred revolutions and raising 800 gallons a minute 90 feet. On looking at it Mr. Thornycroft expected to find a good result; but he had computed the efficiency to be only a little above 0·22. Admiral Selwyn further stated that there was a pressure of 45 lbs. per square inch, and that to that would have to be added 800 gallons per minute discharged on the top of the 90 feet. At first he was puzzled to know how that was to be added; but in taking into consideration the 8-inch pipe, which he described, he found that the water flowed through it at 6 feet per second, and what was really to be added to the 90 feet was therefore the head due to the velocity of 6 feet per second. On working that out he found that it came to about ½-foot, and instead of lifting water 90 feet it was lifted 90½ feet. He did not know what the percentage was, but it was perhaps 22 or 22½; so that he was afraid that the pump mentioned by Admiral Selwyn did not compare favourably with his own. The discharge of the pump described by Admiral Selwyn was immensely less than the discharge from the pump of the hydraulic-boat, although the disk was the same size and the revolutions were nearly double. Admiral Selwyn had stated that in what he considered a perfect pump the water discharged had the same velocity as the periphery of the disk. That, he thought, was entirely illusory. In one case it might be nearly correct. If only a little water was allowed to flow through a large pump, so that the water was a long while in the disk, no matter of what shape the vanes were, the water in the disk attained practically the same velocity as the disk of the pump. He believed that when the flow was limited in that way all pumps gave about the same lift with the same speed. He had exhibited some diagrams in which the form of the blade had been considered. In the case of the hydraulic-boat that he had constructed, the discharge was large and the motion of the water through the disk was rapid; therefore, the effect of the blade form

Mr cro

was very manifest. In considering the effect of the shape of the blade it was necessary to take into account the speed of the water from the centre to the edge of the disk. The result of the diagram was to show that, taking into account the form of the blade, the water flowed through the pump at just the velocity it ought to do. In the pump of the "Waterwitch," the sectional area of the channel was so large that the water made a turn and half another turn round the disk before leaving the pump-disk. The result came out that the velocity of the periphery was 29·6 feet per second, and the velocity as measured in an equal interval of time was 29 feet. It was exactly 29 in the Author's figures, which was perhaps too close a coincidence. In the case of the Swedish pump the result was not so coincident, the velocity being only 21 out of 23 feet per second. Admiral Selwyn, therefore, was incorrect in saying that a perfect pump was a pump in which the velocity of discharge equalled the velocity of the periphery. He might say further, that if instead of using the radial blade a scoop blade throwing the water forwards were used, under favourable conditions it would possibly make the discharge equal to double the velocity of the periphery, it being assumed that friction might be neglected. It would, therefore, be more than a perfect pump, because by Admiral Selwyn's estimate it would be a pump with an efficiency of 2. Captain Heathorn had stated that the channel screw was invented by the late Mr. Robert Griffiths. He believed that Mr. Griffiths did put a screw in a tube; but the idea of putting a screw in a tube with vanes to alter the direction of the water from the spiral to a direct run out was due to Mr. Arthur Rigg. He put curved vanes behind the screw, and so got some advantage; but he did not contract the channel as the velocity was increased, and in that way secure the full benefit from the arrangement. Admiral Selwyn had calculated the re-action of Mr. Gwynne's pump at about 2 tons. That was surely too high, because he found that a cylindrical column of water 90 feet high and 8 inches in diameter would weigh about 1 ton. In a jet-propeller double that, or about 2 tons, would be obtained if it issued at its own speed, but in the case in question it only issued at 6 feet per second. If the water would issue as slowly as that, and give all the re-action, it would be correct; but, as it would escape at a much greater speed than that, the water would not supply the nozzle. Admiral Sir George Elliot had referred to the "Waterwitch," and to the way in which the water was led to the propeller. He believed his statement was correct, that the arrangement for leading the water to the pump through a canal was inefficient.

Mr. Thornycroft. Where the water entered through parallel plates the velocity was not converted into pressure. In order to convert the speed of a stream into pressure it was necessary that it should enter the small end of a tapering canal. He was not surprised that the channel was found from its surface friction to retard the effect of the propeller. Admiral Sir George Elliot had stated that the discharge seemed to be independent of the size of the nozzle. But Mr. Thornycroft thought there must have been some inaccuracy in the measurement of the speed with which the water had been ejected. With reference to the question of stopping a ship in its own length, it should be remembered that was quite a different thing from stopping a boat in one length. It appeared to be intended to put very large powers in ships; but in the case of a merchant steamer steaming at 14 knots per hour it was admitted that in starting the vessel the propeller was acting efficiently. A large steamer that would go at 14 knots per hour took a long time to attain full velocity, and if the propeller acted efficiently he supposed it would take the same time to stop. Admiral Sir George Elliot considered that the trial to which reference had been made had not been a fair one, and that the boats could not be fairly compared. Nevertheless, the best had been done, and even looking at it at the present time, after seeing where a slight failure might have happened, he did not know that if the work had to be done again it could be done much better. He thought it must be admitted that the discharge being above water-mark was a very great inconvenience. Whether the water could be discharged without loss below water-mark he was not sure. It would involve carrying a great deal more water within the boat, and it would have to be taken obliquely out of the skin. If the present nozzles were projected into the water so that they were forced through the sea he was sure the loss of speed would be very great.

Mr. Rennie. Mr. G. B. RENNIE remarked that the extraordinary speed of 17½ knots per hour in the case of the last torpedo-boat had been arrived at after a great many trials; the speed of the first boats of the same size had been only about 14 knots, so that the comparison was hardly a fair one. In trying various forms of pumps several years ago, in experiments for some large pumping machinery,[1] he found that the efficiency of the "Swedish" pump far exceeded that of the "Thornycroft," which was very much like the pump described by Professor Rankine.[2] The total efficiency of the pump and engine varied from 0·30 to 0·50. He did not think it got so low Mr. as 0·25, as in the cases alluded to in the Paper. The way in which the calculation was made was this: the dock was full, and it was pumped out; the level was taken every ten minutes, and the HP., and the total quantity delivered was divided by the HP., the result being from 0·30 to 0·50 of efficiency. In the case of a propelling-ship he could not see why that should be very much varied if the proportions were suitable. It had been stated that one of the problems to be solved in the hydraulic-propeller was to get sufficient water to propel the vessel. In the case of the "Viper" it appeared that about five times the volume of water was discharged or pushed aft as compared with that in the "Waterwitch." That had always seemed to him a great difficulty with the hydraulic system. About three years ago his firm was asked to supply a floating-dock at a certain distant port. There was a great difficulty in re-erecting the dock abroad, and towing it was considered to be an impossibility. The question was how it was to be propelled. A screw-propeller seemed to be inadmissible from the draught of water, and from the inconvenience of a screw-propeller when docking a vessel. They therefore turned their attention to the question of hydraulic propulsion, taking the "Waterwitch's" experiments as a basis. He had a model showing how hydraulic propulsion might in such a case be advantageously employed. There were a great many compartments in the dock, and a great many pumps for emptying them. It was proposed to increase the pumps and power of the engines, and to make six apertures on each side adapted for propulsion. By that means a fair proportion of propelling area was obtained by the water discharged. The total area of all the openings was about the same as the combined area of one or two or three screw-propellers which would be necessary to propel a vessel, having a given section and capable of carrying a given tonnage, through the water. He thought that in the case of vessels of very light draught of water, and short broad vessels carrying heavy weights, the application of a screw-propeller was almost an impossibility. But while in the one case it might be impossible to apply the screw-propeller, in the other case, namely by hydraulic arrangements, it was possible to propel a vessel at a certain speed. It would be interesting if the Author would explain somewhat more fully the instrument to which he had referred for taking the pressure of the discharge at the nozzles.

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[1] Transactions of the Institution of Naval Architects, vol. xxiv., 1883, p. 126.

[2] "The Steam-Engine," p. 190.

Mr. Edwin N. Henwood considered the hydraulic-propeller so Mr. superior to the screw, that it would be unwise to think any money

Mr. Henwood. wasted that would enable its advantages to be brought out, and its efficiency to be developed. If a like amount of money that had been expended on the screw-propeller were to be expended on the turbine, he maintained that the success of the latter would be secured. With reference to the comparison of the "Viper" and the "Waterwitch," the former was a very objectionable structure; the "Waterwitch" was nearly as bad, but even in the latter the turbine did not have its advantages developed; it was fitted horizontally, and hence one of the streams from the discharge pipes had to be turned by a curved pipe into its proper path. The mode adopted of measuring the velocity of discharge was thoroughly unsatisfactory, as had been remarked in the Paper, since it was well known that a patent log must be carefully kept out of disturbed water in order to get a true result. Nor did he admit that the arrangement described of a pressure-plate $1\frac{5}{16}$ inch square, supported by a more or less knife-edged lever, could be considered entirely trustworthy. With regard to the efficiency of a pump or other machine, it was needless to introduce midship section or displacement coefficient into the equation. When the designers fixed the diameter of the disk, and the number of revolutions of the engines, they had fixed the maximum velocity of discharge possible, and therefore the speed of the vessel; and no reduction of the nozzle, with the object of increasing the velocity of discharge, would materially influence the effective thrust. It appeared to him that the experiments were so devised—in error, no doubt—as to ensure no greater result than that obtained; and the Paper really seemed to lead to a double dilemma. Either the "Waterwitch" pump was effective within $\frac{3}{10}$ of unity, or the feed was an important element in the total velocity of discharge. If the former proposition were true, then there was, under circumstances easy of repetition, no loss whatever in the pump, while in the screw there was always a large percentage of loss, without taking into account pitching, &c. If the latter were the correct explanation, not only was the result from the velocity of feed quite as great as in the "Thornycroft," but the velocity of discharge was much better for the revolutions and the disk-diameter. The revolutions in the "Waterwitch" were forty per minute, and in the "Thornycroft" four hundred and twenty-six; the diameter of the disk in the "Waterwitch" was 14 feet, and in the "Thornycroft" 2½ feet. The velocity of the periphery in the "Waterwitch" was 29·3 feet per second, and in the "Thornycroft" 56 feet. The velocity of discharge in the "Waterwitch" was 29 feet, and in the "Thornycroft" 37·2 feet per second. If the velocity of discharge was more than had

Mr. Henwood.

been observed, as seemed probable from her speed of about 10 knots per hour, those figures would have greater prominence. He was unable to find any reason for neglecting to drive the turbine at a higher velocity. The propeller in the "Thornycroft" boat was driven at six hundred and thirty-six revolutions per minute, and the revolutions of the turbine had been reduced to four hundred and twenty-six. He could not imagine why there should be any objection to driving the turbine at a higher velocity. He should like to ask if there was any objection, and, if so, what, to working the turbine in a vertical direction so as to discharge the water underneath the bottom of the vessel? Allusion had been made to the great difficulty in utilizing the turbine for keeping down any leak. He thought the difficulties were of a very slight character, and could easily be overcome. He wished also to ask whether the speed of the circulating water from the condenser was as great as that of the pump discharge, and whether the discharge was directed aft. He thought the superiority of the turbine-propeller over the screw-propeller in the case of fouling was very great. Everyone who had gone to sea would know what would happen if the propeller broke by meeting a floating substance. In a torpedo-boat propeller driven at six hundred revolutions a minute this would often happen. In the case of the turbine there was no such source of damage.

Captain Curtis.

Captain J. D. Curtis, R.N., understood from Captain Crozier, who had charge of the experiments, that the performance of the "Waterwitch" was very good except when she got into a double-reef-topsail breeze. He presumed that this was on account of the pitching of the vessel. He looked upon a turbine as a kind of endless rope. Admiral Sir George Elliot had referred to the "Viper" being put 2 feet by the stern and going ½ knot per hour faster. The "Serapis," when put 2 feet to the stern, attained the same speed, 10 knots, with an expenditure of 50 tons of coal per twenty-four hours, as she did with 80 tons on an even keel. Reference had been made to the benefit to be derived from having an arrangement by which the screw should not foul. About two years ago the late Mr. Robert Griffiths took out a patent which he called the shield patent. This shield consisted of two plates, so arranged that they prevented the propeller acting on the dead-water, and increased the speed of a ship from 6 to 8 per cent. In addition they prevented vibration, improved the steering, and reduced the racing when the ship was pitching. The following results had been obtained from a steam-launch to which it was

Captain Curtis. applied, the owner Mr. C. Bourrot, and Captain Curtis, R.N, being present at the trial:—

—	Without Shield.	With Shield.
Mean speed in knots per hour	6·417	6·636
„ revolutions per minute	289	265
„ pressure of steam in lbs. per square inch	45·1	42·5

showing a gain of 3½ per cent. in speed, with a saving of 15 per cent. of power.

The shield was inexpensive, the estimated cost for a propeller 12 feet in diameter being only £12, and it could be put on without docking the ship. Taking the cost of marine engines at £40 per nominal HP., and allowing a similar sum for coal, also for the space occupied in the ship by the coal, and for repairs, &c., the increase of 6 per cent. in speed, to obtain which 19 per cent. more power would be required, was equivalent to an increase in the value of the ship of £1,520 for every 100 indicated HP. of the engines. In this shield the upper segment went home to the stern of the vessel; the lower segment over one-half the length of the screw from aft forward. Four trials were made with a small vessel above Teddington Lock, two with the shield and two without, and Mr. Robert Griffiths stated that the shield effected a saving of 15 per cent. out of the 45 per cent. which Mr. Froude asserted to be lost by the screw. How was that? When the vessel went through the water, the water must follow the ship, or the ship could not make any progress. The use of the screw detracted from the power of the water to follow the ship, and that had not been sufficiently thought of and acted upon. The shield patent must not be confounded with the cylinder, as they acted differently, the former allowing the escape of water aft. Directly the screw went down, the water came through between the apertures of the shield, and the following water flowed in to occupy the place of the water displaced by the screw. The screw did not carry the load of water round, and did not curtail the amount of in-flowing water. As the screw came up to the segment of the shield, it cut the volume of water carried round in the ordinary screw, and there was no vibration. Mr. Robert Griffiths used to say that he considered the screw a hydraulic pump, and that for efficiency it

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Captain Curtis.

was placed too near the ship's run. By another patent he placed the propeller two-thirds of its diameter away from the foremost stern-post, and in this way allowed the following water to act upon the ship, and did not detract from the speed of the vessel. In conclusion he might observe that aquatic birds struck out alternately with their feet when swimming, economizing power, the thrust not interfering with or obstructing the following water; and that he had heard a Yarmouth fisherman when giving directions to the builder of his smack, say "Give her a cod's head, and a duck's tail for the stern."

Mr. N. Barnaby.

Mr. N. **Barnaby**, C.B., Director of Naval Construction, said the discussion on the Paper was remarkable for two or three reasons, one of which was that amongst those who took part in it was the gentleman who, forty-five years ago, had patented the mode of propulsion in question. Another was that for the last twenty years the Admiralty had been endeavouring to get some sort of success with the jet-propeller, and that they had not been able to point to any success. A third reason was that he believed there did not exist at the present moment any vessel in the mercantile marine employed on a passage of any kind which was propelled in that manner. Accordingly he expected that when the Paper was read he should have been called upon to explain how it came about that the Admiralty still considered it right to go on trying the jet-propeller; but he presumed, after the speeches which had been made by the gentlemen who had been influential during the last twenty years in urging upon the Admiralty the various trials of that mode of propulsion, that it would not be necessary for him to explain further why the Admiralty were desirous, after the "Waterwitch" trial, to experiment on the jet-propeller again in the boat which had been described. The arguments advanced by the gentlemen to whom he had alluded were to the effect that in ships of commerce many accidents to which the screw-propeller was liable would be altogether avoided; that many of the perils now incidental to navigation would be removed; and that navigators would have in the possession of great pumping-power a means of preserving ships and lives which they did not now possess. And they not only considered that that held good with regard to ships of commerce, but that there were special advantages to ships of war which made it still more desirable that the Admiralty should encourage the use of the new method. For his own part he did not believe in pumping-power. He believed that ships would be saved from perils of the sea by proper building, by proper division into compartments, and not by pumps; and even if they passed from the pumps with

Mr. N. Barnaby. which they were familiar to the pumps which were supposed to be possible under the new system of propulsion, he thought they would still find that they were going in a wrong direction. But under any circumstances, whatever the advantages might be in the directions that had been pointed out, it was clear that if 2 lbs. of coal were to be burnt instead of 1 lb. for getting a certain result, it was, for the purposes of commerce, entirely out of the question to expect the jet-propeller to supplant the screw-propeller, or in any way to come into the field for practical adoption.

With reference to the contract into which the Admiralty entered with Messrs. Thornycroft and Co. for the object that had been described, the Board of Admiralty knew that they might expect a loss of about one-half the power, but they were prepared for that for the sake of the merits which it seemed to them it was possible that such a boat might have. The contract was to give the same indicated HP. as was given in the boat propelled by the screw, namely 100; they were also to guarantee that the machinery worked well; they did not guarantee any speed. As a matter of fact the machinery gave about 160 instead of 100 HP.; and they had secured a boat which he believed would be found by the trials at Portsmouth to possess so many good qualities that it was very unlikely that they would be called upon to alter the machinery back again to the screw-propeller. It was possible that some considerable improvement might be made in the pump, perhaps in the size of the nozzles, and it was possible—he hoped it might be proved to be the case—that there would be a sensibly better result than that which had been already obtained. The boat had the remarkable property of stopping very quickly, but she went astern badly and would not steer—two grave defects. The Author had clearly shown the reason for the losses which had to be sustained in that mode of propulsion; and not merely had he put before the members in a plain manner the whole of the facts in the case, but he had demonstrated this remarkable circumstance—that there was a screw-turbine, a turbine-boat it might be called, the turbine being put outside the boat, giving, with the light draught of water which it was supposed was only suitable for the hydraulic-propeller, the same speed as could be got with the screw-propeller in the ordinary way. The result of the Admiralty experiment, therefore, was a fair boat capable possibly of being considerably improved; an excellent explanation of the causes of the loss in the jet-propeller as compared with the screw; and an entirely new propeller, having admirable qualities. It might happen during the time he remained as an adviser of the

Board of Admiralty that that department might again be pressed, as they had been during the last twenty years, to build a ship propelled by the hydraulic-jet. He could only say, and he said it without the least hesitation, that such a proposal would have his uncompromising opposition. Mr. N. Barnaby.

Mr. Hearson. Mr. THOMAS A. HEARSON said that in the hydraulic experiments under discussion the short-coming of the propeller was due to losses in the flow of the water through the pump and through the passages, and the loss was much more than could be submitted to with a continued use of the propeller. But the system had its advantages, and its advocates said that if experiments were continued the efficiency would be improved. It was therefore advisable, before continuing the experiments or condemning the system, to examine into the loss quantitatively and see how much was inherent in the system and unavoidable, and how much might be got rid of by improving the apparatus. The Author had shown, and it might be accepted without doubt, that 1 ton of water was delivered per second by means of an engine of 167 HP., with an efficiency of 0·77, which amounted to this—that every pound of water delivered received an amount of energy of 31½ foot-pounds, in other words, received a head of 31½ feet. Of this head it might be easily calculated that only 10·4 was usefully employed in propulsion, and that 4 feet more was wasted in the energy with which the water left the nozzle. This left 17·1 feet of head wasted in some other way. If the water were imagined to be taken up gradually by the action of the pump, to be endowed with energy given to it by the wheel from the engine, and to be delivered without any abrupt change of velocity into the casing, and then delivered through the nozzles; in other words, if the pumps were assumed to be acting perfectly, how much loss would there be? The loss in such a case—that due to surface-friction—was calculable. It might be expressed as a fraction or a multiple of the energy of motion at some point in the line of motion, say at the nozzle. He had calculated that in the "Thornycroft" boat the fraction of the energy of motion relatively to the nozzle which represented the loss by surface-friction was approximately 0·4; in other words, the coefficient of resistance as employed in hydraulics to express the loss of head was approximately 0·4, not less than 0·3 nor more than 0·5. Mr. Thornycroft had succeeded in keeping it down almost to the lowest amount; the boat employed lending itself to a comparatively small coefficient of resistance, for the dimensions were small relatively to the power, and the length of the passages were short relatively to their section. Taking

Mr. Hearson. 0·4 of the energy of motion as an accurate estimate, that would amount to 8½ feet of head lost by surface-friction, and there remained yet 8½ feet to account for. It would be instructive to work out the loss and efficiency if a value of 0·3 and also 0·5 were assumed instead of 0·4; and further to work it out for a different velocity of flow. In the "Thornycroft" boat the velocity of flow was 1¾ that of the boat; what the Author called the acceleration was ¾ of the velocity of the boat. If the area of the nozzle was increased 17 per cent., so that the acceleration was reduced to ½, the efficiency fraction was but little altered, being increased from 0·458 to 0·465, calculated from—

$$\text{Efficiency} = \frac{2\frac{s}{v}}{(1+\text{F})\left(1+\frac{s}{v}\right)^2-1};$$

which was derived from—

$$\text{Efficiency} = \frac{v\,s}{vs+\frac{s^2}{2}+\text{F}\cdot\frac{(v+s)^2}{2}}$$

F being the coefficient of resistance, showing that very little indeed could be gained by increasing the size of the nozzle. The best efficiency worked out to 0·52, which was about the limit attainable by this method of propulsion, leaving out of account the efficiency of the mechanism of the engine. If that was good enough as compared with 0·65 of the screw, it might be continued in use; but he thought it would be admitted that it was not good enough. He wished to speak a little more of the loss of 8½ feet, to which he had referred, and which had not yet been accounted for, the additional loss over and above that due to surface friction. From the principle of momentum—the turning moment being equal to the change of moment of momentum imparted to the water—the energy delivered to the water by the engine per lb. of water might be otherwise calculated. It was equal to the product of the velocity of the periphery, and the tangential velocity of delivery divided by G, and equating that to the otherwise estimated amount of 31½ feet of head, it resulted that the water was delivered from the pump wheel with a tangential velocity of but little more than 18 feet per second, from a wheel going at a velocity of 56 feet, into a casing where the water had a much higher speed of motion, since it had on the average a velocity of 37¼ feet. It was the eddying due to the difference in the velocities of the streams in the casing that produced the additional loss of 8½ feet—

Mr. Hearson.

a loss which it was hoped might be got rid of by a better pump. The water came from the wheel into the casing with energy of motion given to it by the engine equivalent only to 5 feet of head. The rest of the 31½ feet was pressure head which took effect in increasing the flow, accelerating the speed of the water in the casing, and the remainder was dissipated in fluid eddying. If it were imagined that the streams were kept separate, and the loss for a moment suspended, then the additional 8½ feet would be employed in increasing the flow of the water. Increasing the flow would mean retarding the tangential delivery into a casing where the water would be going faster, which would increase the tendency to eddying. The pump gorged itself. It was easier to find fault than to suggest a remedy; but he would mention that to enlarge the dimensions of the casing might be an improvement, so that the water might flow round the casing with a smaller velocity and not be subject to the eddying described. To fix guiding blades in the passages leading to the wheel might be another improvement, so that the water would come to the wheel with a considerable moment of momentum, and less would be required to be given to it by the pump-wheel itself; or perhaps by making the blades more radial, less curved back, the loss would be less.

Captain Crozier.

Captain H. E. Crozier, R.N., observed that as the officer who commanded H.M.S. "Viper" in the experimental trials against the "Waterwitch," he begged to take exception to some of Admiral Sir George Elliot's remarks. He had never imagined that any comparison could be made between the ships. He had watched the "Waterwitch," and it appeared to him that unless she used enormous power, she had very low velocity. When off Portland on her passage to Plymouth in a single-reef topsail breeze, and in the trough of a sea, her propelling force nearly failed. Admiral Sir George Elliot had stated that the vessels "Viper," "Vixen," and "Waterwitch," on account of their rolling, were condemned as unseaworthy; and that the two former had been taken out to Bermuda and laid up there. The "Waterwitch" was laid up in ordinary at Plymouth, because she was condemned as unseaworthy after failing so signally off Portland, when she just managed to get in "by the aid of her sails" under the breakwater, was taken to Plymouth when the weather moderated, and was paid off. The "Viper," on the contrary, was employed on the coast of Ireland and at Liverpool. She steamed about 2,000 miles, exposed to uncertain weather, and then made a passage to Bermuda by sail and steam averaging 6 knots per hour. If she had been condemned as unseaworthy it was scarcely possible that the Admiralty

Captain Crozier. would have allowed her to make such a voyage. At the trials between the "Viper" and the "Waterwitch" at Portsmouth, the vessels started with all their weights on board. His orders were that the mean draught of the "Viper" should be about 12 feet 4 inches. He had an idea that by altering the trim, which he was entitled to do, he should get more speed—a more solid body for the screws to work in; and, at the same time, that by taking the weights from the extremities the ship would be more lively and seaworthy. The builders' trial was 9 knots, but he obtained, as he expected, 9·4 knots per hour. Mr. Ruthven, who was on board, complained that the trial was unfair. The Admiralty decided that another trip should be taken, and he then got 9½ knots nearly. He had stated that at the trials the "Viper" exerted HP. up to 650, while the "Waterwitch" exerted 750. The "Waterwitch" had then part of her weights taken out, and she ran, not in such a state of efficiency as she should be in when going to sea, at 9·2 knots per hour. He thought that if a vessel so built managed to attain the speed he had mentioned, the same power would be far better exerted by the screw, which was more simple. Again, what would be the use of the "Waterwitch" in shallow water? If she went into 12 feet 4 inches depth of water, drawing 12 feet 2 inches, she would suck up mud and stones, and remain as if at anchor. As far as her propelling power was concerned, a vessel should go into action under command. Imagine the "Waterwitch" at full speed, at which alone she would be serviceable, struck by a shot, and some of her pumps getting out of order! It had been mentioned that the "Vixen" was condemned when she first went outside the breakwater. That was easily accounted for. It was because her hatches were not battened or screwed down. She nearly sank as the water got into her. Of course, when the bottom of a vessel was flat, and she had 2 feet depth of water in her, as she rolled the fires would be put out. When he took the "Viper" from Plymouth to Bermuda he was thirty days at sea, and there was very little rolling. It was not even necessary to have "fiddles" on the table to prevent the china and glass falling off. In all the 6,000 miles that he had steamed in the ship he had never seen her roll heavily.

Mr. J. R. Ruthven. Mr. J. R. RUTHVEN observed that the Author gave the efficiency of the hydraulic in the "Waterwitch" at 18 per cent., which was a force of only 4,800 lbs. to propel the ship: but to propel the ship at 9·3 knots per hour the hydraulic-propeller must have given out a force of 13,000 lbs., and so have had a total efficiency of nearly 50 per cent. of the indicated HP. A considerable difference

Mr. J. R. Ruthven.

existed in the proportions of the boats whose trials were under comparison. The "Viper," "Waterwitch," and Swedish boats were all about five times the beam in length: while the "Thornycroft" was nearly nine times. The Author had taken the coefficients from the displacements, but when the proportions differed so much the coefficients from the cross-sections furnished a better comparison of the propellers. They were as follows:—

H.M.S. "Viper"	424
H.M.S. "Waterwitch"	357
Swedish screw	277
„ hydraulic	171
"Thornycroft" screw	362
„ hydraulic	160

By these coefficients the "Thornycroft" screw was shown to be inferior to the screw in the "Viper," still more so to the Swedish hydraulic propeller; and the hydraulic propeller in the "Waterwitch" was 123 per cent. better than the "Thornycroft."

Mr. Bernays.

Mr. Joseph Bernays stated that he had had considerable experience with centrifugal pumps, but he had not tried to adapt them to the hydraulic propulsion of vessels, because he considered the principle on which they acted was not suitable for that purpose. He did not say that they could not be used; the examples under discussion showed the contrary; but in matters of that kind the question was one of final efficiency or economy. It was understood with regard to the propulsion of vessels that, in order to obtain the best effect, as much water as possible must be driven towards the stern of the vessel, and with as little speed as possible, otherwise a great deal would be lost in energy. The centrifugal pump propelled a small quantity of water with great speed towards the stern. Comparing the two classes of vessel illustrated in Plate 1 the matter would be rendered clear by merely taking the area of the screw and the area of the inlet of the propeller. Judging from the load-line, he imagined that the diameter of the screw was about 3 feet; the area of the circle would be about 7 square feet; the diameter of the inlet of the hydraulic-propeller was probably 1 foot, and the proportion of 0·785 square foot to 7 square feet represented roughly the difference in the volumes of water acted upon, and showed the necessity of greater speed being imparted to that issuing from the hydraulic-propeller than to that which issued from the screw-propeller. Another disadvantage would necessarily be the position of the pump at the bottom of the vessel, where if any damage occurred it would be very difficult

Mr. Bernays. to remedy it. It was stated in the Paper that floating bodies were not liable to damage the internal propeller, but in reality there was a great sucking in of water into the propeller with a certain amount of speed, which made it liable to catch floating bodies, such as wood shod with iron, and things of that kind. If these were to enter the propeller it would be liable to be damaged, and although it was in sight of everybody, it could not be got at. If in that way the casing were damaged, he imagined that without bulkheads the vessel would be lost. Something had been said about the great power for stopping leaks, but he did not see where it was to come from, or how it was to be applied, any more than with the ordinary screw propulsion. But assuming that hydraulic-propellers were suitable for the propulsion of vessels, he did not think they had had a fair chance in the arrangements that had been exhibited. Perhaps the preference should be given to the shape of the Ruthven propeller, because even in the Paper it was credited with 0·50 efficiency, whereas the more modern arrangement was only credited with 0·47 or 0·46. He did not know whether with regard to the latter any allowance had been made for the improved inlet, because if so the efficiency of the propeller proper would be still less. Many years ago he had written some letters, which were published in some of the engineering papers, with reference to the proper shape of pump-arms. He would not repeat what he had written, but would merely say that the shape of the path which a drop of water should pursue in the pump was a curve like that stated to be followed in the Ruthven propeller, but not so long; and in order to make the water take that course, the arm must wedge itself under the water near the inlet, and must be more radial as it got out, to let the water issue from the pump-disk in a more tangential direction. He spoke of the matter without any prejudice in favour of one pump more than another. The arms forming the real propelling part of the pump, their shape was of great importance. The position of the pump, as shown in the drawing of the torpedo boat (Plate 1), appeared to him to be altogether wrong. He thought that there were many places where the power of the engine given out was wasted. First, the water was raised vertically or on an inclined shoot, then it was bent at right angles and round the casing in all directions, whereas it was only wanted to move in one direction. Next it had to go through several small pipes, each bent, and finally it issued through a small jet. At every such change the water lost some of the power that had been put into it. An endeavour should be made to deliver the water out of the pump in as broad a stream as possible, so as to issue with the least possible speed and with the least possible loss of effect; and that, he believed, if other difficulties were not realized, could be done by placing the pump as far astern as possible. How that might affect going astern or steering he would not say, but he thought there might be methods of getting over the difficulty. Mr. Bernays.

Mr. W. ATKINSON observed with reference to the intake of water by the inclined shoot, that the Author stated that in all previous hydraulic boats the water had been taken through a hole in the bottom, in such a way that all its velocity relatively to the ship was destroyed before it entered the pump, and that that velocity had to be restored by the pump, which involved a large waste of power. It appeared to him that there was a misconception upon that point. In the first place it must be assumed that the water in the river or the sea was stationary, and that the boat only was in motion. As the water was not in motion, it was evident that no energy had been put into it, and that there was no loss to avoid. If it was thought necessary to adopt a scoop in order to bring the water up, then it would be merely a question whether that scoop had better be used in that particular position, having already employed the power of the engine or of the machine to propel the boat; or whether it was better only that the pump itself should draw it up without the scoop. Credit was taken for the fact that in the arrangement described the relative velocity of the water and of the boat was not lost. It was, however, he thought, manifest that if the scoop were taken a little further up, and brought to the vertical line, the water would have acquired the velocity of the vessel before it entered the turbine. It had arrived nearly at that position, and it was evident that the greater portion of the velocity of the vessel would have been given to the water, and if it could not be utilized afterwards by changing its motion, all that energy would be lost. But it was not lost; it was merely the portion due to friction which was lost. If the turbine was lowered almost to the level of the bottom, the scoop done away with, and the hole for taking up the water elongated, then the lifting power of the pump would virtually do all that the scoop did, and this advantage would follow, that the motion of the vessel would not have been communicated to the water; part of the water would not have motion communicated to it, and a certain amount of loss would be avoided. It appeared to him, therefore, that the scoop was a mistake. With reference to the chamber for the distribution of the water into the vanes of the propellers, it would Mr. Atkinson.

Mr. Atkinson. be seen that in the model of a parallel-flow turbine exhibited there were guide blades, the object of which was to transfer the pressure of the water into rotary motion, so that when it entered into the chamber of the pump the inner portion of which was rotating perhaps at about 30 feet per second, it should enter without shock and loss of power. He should like to know why the guide-chamber had been omitted, because he had never known any turbine arrangement without one. He thought if it was desired to perfect the pump, an efficient guide-blade chamber should be introduced, and at the top an inverted cone, as was shown in the diagram of the "Waterwitch" pump, where, however, it was used for a different purpose. There would then be a second guiding power. The water which was forced in a state of great commotion into the chamber would be let off gradually in each direction. It had been stated in the discussion that the object was to drive as much water as possible astern. He thought that the object was not to send the water astern, but to propel the vessel ahead. In driving water astern, the energy due to the motion greater than the motion of the water in the sea was lost. The Paper stated that the velocity of the vessel was 22 feet per second, and the velocity of the jet 37 feet. He supposed that the difference between those two velocities was equivalent to 16 HP. lost. He believed it had been admitted by Mr. Thornycroft that it would have been better not to have had the nozzles at so great an elevation. If the water by that arrangement was raised only 2 feet it was equivalent to a loss of 8 HP. That, he thought, would indicate the direction in which there were great and unnecessary losses. If it were really necessary to have that particular form of hydraulic propulsion, he thought it would be much better to place the axis horizontally, which would no doubt get rid of a great deal of choking up of the passages, and there would be a direct flow of the water through the propeller, and economy would be the result. The axial or parallel propeller appeared to be the best. He wished to direct attention to a turbine with an inward flow, that had, he believed, yielded the highest effectiveness of any turbine ever constructed, and no doubt it could be appropriately placed in a vessel of the kind described, and would give much greater efficiency than the one under discussion. Probably one such turbine on each side of the boat would be the best arrangement. It was an American invention, and called the "Victor," being a modification of the "Hercules." The guide-blade chamber formed the outside casing, inside which was the register rate and the vanes as in an inward flow, but with the addition of another set of vanes as in a parallel-flow turbine.

Mr. Atkinson. Thus within a small space there was a large vane-surface, with the result that for the same weight a greater power could be developed than in any other turbine. Drawings of it had been given in Emerson's "Hydro-dynamics," 1882, p. 124.

Mr. Rich. Mr. W. E. Rich observed that the Author had quoted the statement of Lord Dufferin's Committee: "We are of opinion that the system is deserving of a more thorough trial than it has yet received"; and he thought if the Institution of Civil Engineers were called upon to advise upon such a subject at the present moment, they would have to endorse those remarks made thirteen years ago. He believed that Mr. Thornycroft, who had done a great deal in his first attempt, had not attempted to remodel the vessel or its machinery in any essential particular since his trials were made; and probably if he had to construct another vessel on the same system many points brought out by the experiments would lead him further on in the right direction. The late Professor Rankine laid down the principle which was now generally recognized, that the chief reason why very high efficiencies could never be looked for from the hydraulic system of propulsion was that another machine was introduced whose efficiency must be allowed for. Mr. Thornycroft had already obtained a speed of 12·6 knots per hour, and from a general review of the calculations given in the Paper, Mr. Rich was of opinion that 15 knots was a possible speed with the same boat and engine power. The great point in which improved results might be obtained was, he thought, in the efficiency of the fan. The Author showed an efficiency of fan and engine combined of 0·35, and then, estimating that the engines gave 0·77 efficiency in themselves, he credited the pump with the remaining 0·46. But the Thornycroft engines gave a much higher efficiency than 0·77. The traction engines tested by the Royal Agricultural Society at Wolverhampton in 1871 gave an average of 0·84, and the portable engines tried at Cardiff in the following year gave a mean efficiency of 0·825. He did not think that the engines of the Thornycroft boat were inferior to these, for probably none were made with greater care and more frictionless. Taking the Thornycroft engines, therefore, as capable of doing 0·83, the efficiency of the fans would be 0·42. As long ago as 1851 the late Mr. Appold, with the assistance of other gentlemen, determined that one of his earliest pump-fans, 12 inches in diameter and 6 inches wide, pumping 1,250 gallons a minute with a 10-feet lift, gave an efficiency of 0·68. A centrifugal pumping engine, for draining Whittlesea Mere, was erected in the following year under Mr. Appold's supervision, and twenty

Mr. Rich. years afterwards, when Mr. Rich assisted in testing it, the engine and pump were giving a combined efficiency of 0·45. A larger engine and pump, which were substituted for the originals shortly after these trials, gave 0·53 of efficiency. The Witham drainage engines and pumps, which he tested in 1873, were much more powerful, and gave a combined efficiency of 0·63, which he believed was about the highest result on record. They were throwing from 350 to 400 gallons per minute, as measured over a gauge-weir. Of course, with a centrifugal pump, as with a turbine, there was for every fall or lift one speed, and one volume of discharge at which it would give its maximum efficiency, and if the proper relations between fall speed and discharge were not strictly regulated the efficiency decreased very much. He had not examined the details of the "Thornycroft" fan analytically, nor was he in a position at the moment to offer any advice as to the best way of improving it; but from the experiments of Mr. Appold and others, he was inclined to think that a broader fan with a smaller diameter, and driven faster, would probably give greater efficiency than the fan exhibited. He also thought that if the skin of the fan could be kept further from the skin of the casing, better results would be obtained. In the construction of fans working centrifugal pumps skin-friction was an important element. In the Witham pumps, with fans 7 feet in diameter and 2 feet 2 inches wide, skin-friction absorbed a material part of the whole power. There was not only skin-friction on the circular surfaces, but there was the skin-friction of the water passing through the passages. The result was that a small fan often gave a higher efficiency than a large one. The fan of the Swedish pump resembled that of the late Mr. Appold, but its blades curved more. He had substituted a fan with blades shaped like those in the Thornycroft propeller, about nine years ago, for an Appold fan at a polder pumping-station near Amsterdam. The object in putting it in was to get the same quantity of water lifted with a lower engine speed, and this had been attained; but there was no evidence of improved efficiency in consequence of the change.

Mr. Schönheyder. Mr. W. SCHÖNHEYDER said it was important, in arranging the propulsion of a boat by hydraulic power, to have a pump of the best shape for efficiency. It appeared, from what had been done with centrifugal pumps by various engineers, that there was a great diversity of opinion as to the best form of pump both in the cross-section and in the shape of the blades. He was certain that if engineers would pay attention to what had been already

Mr. Schönheyder.

accomplished with turbines, and would remember that a centrifugal pump was only an inverted turbine, they would not have such poor results as had been from time to time recorded. The best information with regard to the centrifugal pump was, he believed, to be found in Mr. David Thomson's Paper, read on the 14th of February, 1871,[1] which contained an account of what had been done by the late Mr. Appold. Mr. Appold had experimented upon fans with arms of three shapes, radial straight arms, straight arms placed at an angle with the radius, and curved arms such as those in the Swedish pump. Naturally he obtained the best results with arms like those in the Swedish pump, but that was no proof that that was the best possible section. The object, of course, in constructing arms should be to give the water a gradual rotary motion, cutting through it like a knife through a piece of cheese on entering, and then gradually getting the rotary motion, and finally giving it the proper direction in which it was wanted to run. That, of course, was the reverse of what was found in turbines. In the blades experimented with by Mr. Appold, the curve was that of a screw of uniform pitch. The water being moved round by those blades at once acquired a certain rotary motion, and no increase of rotary motion was given by them afterwards. The effect was a blow to the water, which must very much diminish the efficiency, in accordance with what had been laid down by the best authorities on turbines. The "Thornycroft" wheel was a better pump than the Swedish pump or the "Waterwitch." Mr. Thornycroft had shown the path which the water took in passing through the wheel, but unfortunately he partly condemned his own work. It would be seen that the water entering the vanes took first of all a radial direction, or rather a radial direction partly backwards, and when it had passed halfway through the vanes a rotary motion was given to it gradually. From the diagram it appeared as if the vanes were made, he scarcely knew how, and afterwards the path which the water took was found; instead of which, first of all the path of the water should be determined, and then the vanes made of a proper section to give it that path. It appeared as if the first half of the length of the blades was useless, or worse than useless, because the water passed through them, and lost a certain amount of efficiency in friction. If half the inner length of the blades was cut away, he believed that better results would be obtained, motion would be imparted to the water in the same way, and there would

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[1] Minutes of Proceedings Inst. C.E., vol. xxxii., p. 26.

Mr. Schönheyder. be less frictional resistance. He did not know how far the diagram of the "Waterwitch" pump was correct—no doubt it was correct from the data possessed by Mr. Thornycroft, but probably the delivery had been very much underrated. It would be seen that the water received a sudden blow immediately it entered the arms, which it ought not to have, and which would necessarily occasion loss. Mr. Thornycroft was correct in stating that Mr. Rigg was the first to use guide-blades for a propeller, but not in stating that he used curved guide-blades, for they were perfectly flat. The water was assumed to strike against them like a billiard ball against the cushion, and fly off at the same angle. A Paper by Mr. Rigg on the subject was read before the Society of Engineers in 1868,[1] and the honour of first using that method was certainly due to him. As to the possibility of a water-propelled boat ever being efficient he thought there could be no two opinions. Instead of putting a propeller into the water to act upon the whole sea, it appeared to him to be necessary to take the sea into the boat in order to get a sufficient volume to act with efficiency.

Mr. Symes. Mr. J. P. SYMES said that reference had been made in the Paper to a floating fire-engine in which, by the advice of Mr. Brunel, one of the hydraulic-propellers was used. Some of the previous speakers had expressed an opinion that if the axle were placed horizontally it would give better results than if it were vertical; and one speaker had said it was a pity that the hydraulic-propeller had not been tried on a boat of exactly the same dimensions. Messrs. Thornycroft in their experiments had varied the dimensions of the boat somewhat, for the purpose of giving an advantage to the hydraulic-propeller. The floating engine referred to in the Paper was still in existence. It was fitted with a centrifugal pump with the axle horizontal, and was driven by means of gear, but did not exist at the present as a hydraulic-propelled boat. This vessel had been built by Messrs. Ditchburn and Mair in 1855, and the machinery, including the centrifugal pump for propelling the vessel, the steam fire-engine and the gearing for driving the centrifugal pump, were fitted by Messrs. Shand, Mason and Co., the engines being high pressure, but the results obtained were not very satisfactory as to speed. The boat was 100 feet long, 14 feet 9 inches beam, and had a mean draught of 4 feet 4 inches. The design of the boat was very favourable for a moderate speed—a much higher speed than had ever been obtained by the hydraulic-propeller. Water was taken in at the flat bottom through a square

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[1] Society of Engineers. Transactions for 1868, p. 202.

Mr. Symes.

hole without any particular guide. Originally the water was discharged below the water-line, which some persons seemed to think an advantage; but some time after the jets were removed and placed a short distance above, as described in the Paper, but the difference in speed was scarcely perceptible. He was not able to say whether the jet protruded outside the vessel; if it did that no doubt would account for there being no difference in the speed, because the friction of the jet outside would be sufficient to re-act against the advantage of placing it below the water. The speed obtained by the boat, as far as he had been able to ascertain, was never higher than 6 knots per hour. There was always great difficulty in keeping the gear in order, so much so that at last the vessel was towed to fires instead of using the hydraulic-propeller. In the year 1878 the Metropolitan Board of Works, which had charge of the fire brigade, decided to have the vessel converted into a twin-screw boat. Here was a vessel of the same dimensions, with the same displacement and draught of water forward and aft, and by converting it into a twin-screw boat an opportunity would be afforded of seeing if any advantage would be gained. His opinion was that Mr. Thornycroft in altering his design had done so in favour of the hydraulic-propeller. He could not give any indicated HP., but the basis might be taken as boiler power. It was proved that the engines driving the centrifugal pump used all the steam that could be generated by the boilers; and as the boilers were left in, they had the same basis of power. Two pairs of high-pressure engines were fitted in by Messrs. A. Wilson and Co., of Vauxhall, with cylinders 8 inches in diameter and 9 inches stroke, the propellers were 3 feet in diameter and 5 feet 6 inches pitch, from two hundred and sixty to two hundred and eighty revolutions per minute were attained. On a trial with the twin-screws a speed of 10 knots per hour was obtained against 6 knots for the hydraulic-propeller with the same boat, the same displacement, and the conditions in fact almost the same. He thought this confirmed the Author's opinion and experiments that even with exactly the same boat the results would be much in favour of the screw-propellers for the power developed by the engines, so much so that the hydraulic-propeller had no chance in the competition.

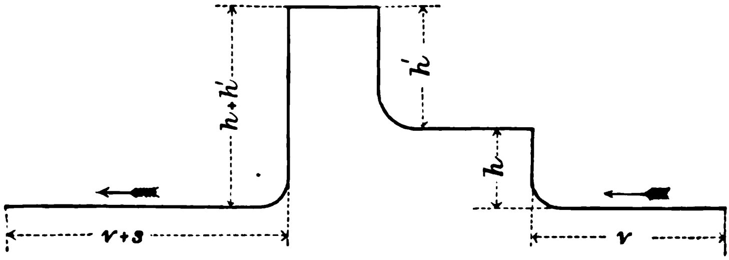
Mr. Barnaby.

Mr. SYDNEY W. BARNABY, in reply, said he thought the Institution as a body had reason to congratulate itself upon not being responsible for some of the facts and opinions that had been advanced during the discussion. So many criticisms had been made upon the Paper, that he should not reply to them all. Of course, with

Mr. Barnaby. a large hole in the bottom of the boat, it would be likely that floating material would get in, but to prevent that a grating had been supplied, and although it checked the admission of the water, it had the advantage of keeping anything out except a certain amount of sand and gravel, which the pump threw out in very shallow water. He could not approve of the suggestion that the pump should be at the stern of the boat. The reason for putting it in the centre was plain; the object was to get as big a pump as possible, and as the boat was small, the centre was the only place for it. There was a further benefit from the extra displacement obtained by the increased section of the boat abaft the inlet, shown in Fig. 7. The further forward the pump was taken the greater the advantage of that extra displacement. It had also been said that the pump would be more efficient if it had guide-blades. The fans of the pump were so adapted as to take the water without shock, and to gradually accelerate it, and he thought when the pump was so devised the guide-blades were not necessary. With respect to his coefficient of efficiency of the engines, he had taken some pains to get at a fair estimate. He had written to Mr. Rich, knowing that he had made many experiments, and he gave him a probable efficiency of about 0·80. Mr. Lilliehöök, the designer of the Swedish boat, had taken his efficiency as low as 0·72; and Mr. Brin, the Italian constructor, about the same. Mr. Wright gave 0·77 as a more probable efficiency for a marine steam-engine. It should be observed that although the "Thornycroft" engine was no doubt as efficient as any other, still in the trials the engines were quite new in the boat (after a little running no doubt they would go more freely), while those of Mr. Rich were running in their most efficient condition. It was, in fact, a trial for economy. With regard to the use of the scoop for preserving the velocity of the feed, the water had a velocity relative to the boat, equal to the speed of the boat. If the water was admitted to the pump by a hole at right angles to the direction of its motion, all that velocity was instantly destroyed. In other words, the water had a forward velocity imparted to it by the boat equal to the speed of the boat, out of which velocity no advantage could be obtained. When the entrance was a curved one, as in the "Thornycroft" boat, the water had its direction gradually changed, and it might be made to rise into a tank at a height depending upon its velocity. True it would have a forward velocity equal to the speed of the boat if the pipe reached the vertical; but it would be the original velocity with a changed direction. It might be carried any distance in the tank, but when it was allowed to fall

through a pipe curving aft it would issue with the same velocity, Mr.
neglecting friction, with which it entered. In Fig. 14—

Fig. 14.



v = velocity of feed;
h = head acquired by velocity of feed v;
h' = head supplied by pump, corresponding to acceleration s;
$h + h'$ = total head;
$v + s$ = velocity of discharge due to head $h + h'$.

In reference to what had been said about the arms of the pump appearing to be ill-adapted to the path of the water as calculated, it should be remembered that a very slight variation in the quantity of water altered that path considerably. The pump was discharging rather more water than was intended, and it illustrated the fact that a turbine could only work at maximum efficiency in one particular set of conditions. The speakers in favour of the system of hydraulic propulsion might be roughly divided into two classes: there were first those who said they admitted that there was a considerable loss of power by the system, but looking at the great advantages which were to be obtained from it (advantages which had been described in the Paper and which he thought might be easily over-estimated), the question was what would they pay for them—would they sacrifice something in speed, or would they keep the speed and make sacrifices in armour, or guns, or cargo, as the case might be? That was a question for the naval architect and the shipowner to determine, and he had nothing to say upon it. But the other class said not only that there need be no loss from the adoption of hydraulic propulsion, but that there should be a gain. Most of those speakers had commenced by saying that they did not approach the subject from a scientific point of view, and they proceeded to lay down a set of novel and astonishing principles as a basis from which to attack the results that had been arrived at. He could only say that he had endeavoured to approach the subject from a

Mr. Barnaby. scientific standpoint. He did not know whether Admiral Sel
was one of those who made that modest disclaimer, but on
the most important, and certainly the most misleading of t
novel principles was that enunciated by him when he said that
velocity of discharge was a much more important thing than
quantity; that the quantity did not matter in the least in re
to the velocity obtained. Taking this principle as a basis
endeavoured to show that by fixing the velocity of discharg
37 feet per second, Mr. Thornycroft had thereby limited the s
of the boat to that obtained. He stated he was sure he could
obtain a speed of 17·7 knots per hour without a velocity of
charge of 91 feet per second. Mr. Barnaby submitted some fig
in order to invite attention to that point, and to show the fal
of the principle thus laid down.

$$\text{Efficiency of jet} = \frac{\dfrac{w\,v\,s}{g}}{\dfrac{w\,v\,s}{g} + \dfrac{w\,s^2}{2\,g} + \dfrac{f\,w\,v^2}{2\,g}};$$

w = weight of water discharged in lbs. per second;
v = speed of vessel in feet per second;
s = slip or acceleration;
g = 32·2 feet per second.

——	"Thornycroft" Boat.	Hypothesis I.	Hypothesis II.
w	2210	221	22100
v	21·4	21·4	21·4
s	15·85	158·5	1·585
$v + s$ . . .	37·25	179·9	22·985
Efficiency . .	0·71	0·23	0·77

At the top was the formula given by Professor Rankine fo
efficiency of the jet. It had been fully described in the Appe
to the Paper; he would therefore only repeat that the first
in the denominator was useful work, the other two terms
lost work. It would be seen that in the second term w appe
in the first power only, while s the acceleration was in the se
power. Therefore, if the velocity of discharge was increase
loss was increased as the square. If the quantity discharged
increased the loss increased at the same rate only. The first co
showed the quantity of water acted on and the rate of dischar
the "Thornycroft" boat. The efficiency was 0·71. In the se
column one-tenth the quantity was assumed, and ten times

Mr. Barnaby.

acceleration, the useful work, and therefore the speed of vessel, remaining the same. The efficiency was 0·23. In this case the indicated HP. would require to have been increased as 23 to 71. In the third column there was ten times the quantity and one-tenth the acceleration, giving an efficiency of 0·77 with the same propelling effect. In these hypotheses no account had been taken of friction, which added seriously to the loss as the velocity of discharge was increased. Therefore the rate of discharge was not so important as the quantity discharged, and if the whole Thames had been pumped through the boat it would only have required a velocity of discharge infinitesimally greater than the speed desired. He had heard that Admiral Selwyn had built a hydraulic boat of his own. It would have been interesting to the Institution if he had explained how he had applied his principles, and with what success. He regretted that the discussion had been confined to the old battle-field of the "Waterwitch" and the "Viper." He did not care to enter into that battle-field. If the results had proved conclusively that jet propulsion was wrong, there would have been no need for a "Thornycroft" hydraulic-boat or any other. They did not prove anything conclusively, and it was not until he came to collect all the information available that he found out why that was so. No experiments of any value had been made as to the quantity of water pumped, or as to the rate of discharge; it was therefore impossible to get accurately at what the duty of the pump was. He did not pretend that the efficiency of 0·18 was absolutely correct. As he had previously explained, he had taken that from the calculations made by Mr. Brin in a Paper read before the Institution of Naval Architects (vol. xii.). From considerations of the probable resistance of a vessel of the size and shape of the "Waterwitch," he calculated that 0·18 of the HP. which was indicated would be sufficient to give the speed which she attained. Mr. Barnaby had been induced to take those figures almost upon faith. Mr. Ruthven, junior, had, he thought, proved too much; he had endeavoured to show that the screw of the "Viper" was a great deal better than the screw of the torpedo-boats, but that was so obviously impossible that the rest of his calculations upon the same basis were probably incorrect. The new boats did really give a fair means of comparison. There had been no prejudice one way or the other, either in regard to the "Thornycroft" or the Swedish boats. In each case the designers of the screws designed the turbines, and the results had been unchallenged. The coefficients which he had given for the "Viper" and the "Waterwitch" were supplied to him by the Admiralty, Mr. Barnaby. and they represented trials where the displacement and the speeds were as nearly as possible alike. In the Tables which Mr. M. W. Ruthven had exhibited, he had mixed up the coefficients of trials where the displacement varied from 995 to 1,205 tons, and where the speed varied as much as 1½ knot per hour; he had mixed them all up and divided them by the number. He would refer to Fig. 13 to show what the effect of that was. In that diagram was the curve of the coefficient of a torpedo-boat at the same displacement under different speeds. The influence of a difference of 1½ knot could be seen. At 9 knots there was a coefficient of 228, and at 1½ knot more speed it had fallen to 160. What was the value of any comparison between two systems of propulsion if carried out upon those two speeds, and if besides the displacements and forms were different? The attempt to make such a comparison betrayed a fundamental misconception of the meaning and value of coefficients of performance. Similar vessels of the same length should be tried at the same speeds and at the same displacements if a comparison of propellers was desired, and similar vessels of different lengths should be tried at corresponding displacements and corresponding speeds. By corresponding displacements he meant displacements varying as the cube of the length; by corresponding speeds, speeds varying as the square root of the length. Mr. Ruthven found a discrepancy between Table 1 and Table 2. He did not understand how the coefficient of the "Waterwitch" could be so much better than the coefficient of the "Thornycroft" boat, and nevertheless the efficiency of the hydraulic machine in the latter boat could be so much better than that of the "Waterwitch." But there was not the smallest connection between the coefficient of performances in Table 1 and the efficiency of the hydraulic machine in Table 2. If the engines of the "Waterwitch" were put upon a Thames barge, or if the "Waterwitch" herself were moored head and stern the efficiency of the hydraulic-machine might be exactly the same, but Mr. Ruthven would not like the efficiency of his system of propulsion to be judged by the coefficients of performance obtained in such a trial. Admiral Sir George Elliot had claimed that jet propulsion had never had a fair trial, and that the designers had always been hampered by the conditions imposed by the Admiralty. If he had led him to believe that they had been hampered in any way by conditions as to the draught of water he had misled him. The only conditions imposed were those stated by the Director of Naval Construction, namely that the boats should be as nearly as possible the same, that a similar boiler was to be used, and that the designers were to do

Mr. B

their best as far as getting speed was concerned. Every suggestion which they had made as to modifications in the form of the trial boat which they considered would be to its advantage had been cheerfully assented to by the Admiralty. Some of the suggestions were undoubtedly prejudicial to the efficiency of the boat as a torpedo-boat, but as it was a trial of hydraulic propulsion the designers were permitted to make even those modifications. One of them, a small one, was in the position of the boiler; it was carried out to the end of the boat, and far away from the steam impulse for ejecting the torpedoes, which was an objectionable feature, but which was considered advisable for other reasons. Another was the position of the discharges. If the discharges had been below water the noise would have been avoided, and also the serious objection arising from the large amount of white water which made the boat readily detected by the electric light at night. They had however fitted up a small centrifugal pump in a launch with the object of determining the best position for the discharges. The nozzles were attached to dynamometers, so that the reaction could be measured. The experiment showed an enormous resistance caused by the nozzle when below the surface; and it was clear that unless long discharge pipes were used carrying the water aft, as in the Swedish boat, where the nozzles were below the surface (Fig. 2), the discharge must be above water. Admiral Sir George Elliot had also said that the screw-boat should have been reduced to the same draught of water as the hydraulic. As a matter of fact the draught of the screw-boat under the keel was less than that of the hydraulic-boat, and it was only the blades of the screw revolving below the keel which made it exceed the draught of the hydraulic-boat. Admiral Sir George Elliot would cut off the legs of the screw-boat, and make it run upon its stumps, in order that it should not outrun the hydraulic. That was not fair, but even on that ground the screw-turbine propeller would beat the centrifugal pump. As had been previously stated, in a second class torpedo-boat, with 4 inches less draught of water than the hydraulic now had, nearly 5 knots additional speed per hour could have been obtained. The little boat, of which a model was exhibited, only drew 12 inches of water, and it reached the astonishing speed of 15 miles per hour with a screw-turbine propeller. A great deal of the uncertainty connected with the subject had been produced by loose statements as to the performances which had been attained by hydraulic-propelled vessels. It had been said that the "Nautilus" had beaten another vessel, the "Volunteer"; it had been stated

Mr. Barnaby. perhaps what the speeds of the vessels were, and their HP., but nothing was mentioned about the displacements, or which had the advantage in point of form. It was asserted, too, that the "Nautilus" challenged all the Citizen boats on the river, and that they declined the contest. That, however, proved nothing. He might have come before the Institution and said, "Here is a hydraulic-propelled boat two-thirds the length of a Citizen steamer, with very much less displacement, with a boiler of only two-thirds the heating surface and a weight of machinery of only 5½ tons against 14 tons, and yet it can run round and round any Citizen boat on the river. That would have been the truth and nothing but the truth, but not the whole truth. The whole truth was that by means of forced draught an enormous power in proportion to her size was realized. It was a great pity that statements should be put forward which proved nothing, but which led to a great deal of misconception. He thought it was most desirable that scientific questions of that sort should be treated in a more scientific manner.

Sir J. W. Bazalgette. Sir J. W. BAZALGETTE, C.B., President, said that if the Institution, as a body, did not hold itself responsible for the views which had been expressed, he felt sure that the subject had been advanced considerably by the discussion which had taken place. He had no doubt that it would lead to further experiments, and that an increase in the rate of propulsion by different modes would be the result of the consideration that had been given to the subject. The members must also feel that the Author had devoted great attention to the subject, and had made a very able reply to the many objections which had been raised in the course of the debate.

[CORRESPONDENCE.

Correspondence.

Mr. O

Mr. J. C. R. Okes considered the efficiency of the pumps both in the "Waterwitch" and in Messrs. Thornycroft's boats low, as compared with that of some pumps. On the 17th of August, 1883, some tests of a Root's rotary-pump, made by Messrs. Mather and Platt, gave the following results, where in one case the efficiency rose to 0·74 of the power applied:—

Results of Tests of a No. 7 Root's Rotary Pump.

Suction- and Delivery-Pipes, 7 inches diameter.

No. of Test.	Revolutions of Engine.	Revolutions of Pump.	Depth of Overflow 2 feet Wide.	Delivery per Hour.	Vacuum in Suction Pipe.	Pressure in Delivery Pipe.	Height Lifted calculated from Pressure and Vacuum.
				Gallons.	lbs. ft.	lbs.	Feet.
1	156	152	5·0	43,185	6 =14	11	39
2	148	146	4·9	41,715	9¾=22½	10½	47
3	136	132	4·4	35,610	4¾=11	22	62
4	160	156	5·1	44,377	7¼=17	9	37
5	110	106	4·0	30,900	6 =14	7½	31
6	176	172	5·6	51,217	8 =18½	8	37
7	116	108	3·3	23,175	12½=28½	20	75
8	160	152	4·9	41,715	10½=24	16	61
9	160	156	4·9	41,715	12 =27½	19	71
10	156	152	4·8	40,552	5½=12½	29	79

No. of Test.	Theoretical HP. required for the Water Lifted.	Indicated HP.	Friction of Engine in HP.	Actual HP. required.	Efficiency of Pump.	Gallons of Water the Pump would deliver, assuming there to be no Slip.	Slip of Water.	Slip of Strap.
							Per cent.	Per cent.
1	8·50	17·6	5·7	11·9	0·714	53,017	18·5	3·2
2	9·90	20·2	5·4	14·8	0·668	50,925	18·0	2·0
3	11·15	21·8	5·0	16·8	0·663	46,042	22·6	3·6
4	8·29	18·2	5·9	12·3	0·674	54,413	18·5	3·0
5	4·83	10·5	4·0	6·5	0·743	36,973	16·4	4·3
6	9·57	22·4	6·5	15·9	0·600	59,994	14·6	3·0
7	8·77	19·0	4·3	14·7	0·597	37,670	38·5	7·5
8	12·85	24·3	5·9	18·4	0·698	53,017	21·3	5·6
9	14·95	27·8	5·9	21·9	0·683	54,413	23·3	3·0
10	16·18	34·2	5·7	28·5	0·568	53,017	23·5	3·2

Mr. S

Mr. J. F. Spencer remarked that Mr. H. Somerset Mackenzie of Falmouth had made a series of experiments on hydraulic propulsion in a boat 42 feet long by 6 feet 6 inches, fitted with a dual

Mr. Spencer. engine, the pump or turbine being two screws, one left one right, working close to each other on a solid and a hollow shaft respectively. The water entered through an aperture 2 feet 6 inches square in the bottom of the hull, and was discharged through two 9½-inch nozzles just clear of the water-surface. With the same engine-power, the speed was increased from about 8 to 10 miles per hour by enlarging the diameter of the nozzles from 7½ to 9½ inches; and Mr. Mackenzie's experiments led him to conclude that volume of discharge was more important for efficiency and speed than velocity of discharge.

Mr. Wilson. Mr. G. WILSON observed that the Author of the Paper, in analysing the results obtained from the trials with the "Thornycroft" boat, used as a basis for calculation the pressures of the jets of water from the outlet-nozzles on a thin plate $1\frac{5}{16}$ inch square. The plate was placed in the jet just where it left the nozzle. Referring to the calculations of reaction when the boat was moored, the Author stated that at two hundred and ninety-five revolutions of the turbine per minute the pressure of the jet of water on this plate was 9·35 lbs., or 8·4 lbs. mean pressure, the latter being equivalent to $\frac{8 \cdot 4}{1 \cdot 3125^2} = 4 \cdot 87$ lbs. per square inch. From that pressure it was calculated that the weight of water forced through the outlet-nozzles was 1,426 lbs., and that the velocity of the water was 24 feet per second. The reaction of the water issuing through the nozzles was given at 1,062 lbs. = 9½ cwt. The pull recorded on the dynamometer attached to the stern of the boat was $9\frac{7}{8}$ cwt. The Author considered that the reaction of the water discharged through the nozzles, and the pull on the dynamometer being about equal in intensity, was a satisfactory test of the accuracy of his method of research.

For the purpose of ascertaining the pressures of the water issuing through the outlet-nozzles of pumps, Mr. Wilson had tried various methods, one of which was similar in principle to the Author's; but he never obtained reliable results except in actually measuring the quantity of water ejected through the outlet-nozzle in a given time. The method adopted was simply to set a tank or other receptacle in a suitable position close to the nozzle of the pump. An open trough or gutter was placed underneath the nozzle in line with the issuing stream of water in such a manner that the water flowed along the trough. At a given signal the end of the trough was instantly shunted so as to cause the water to flow into the tank. At another signal the end of the trough was moved away from the tank. Between those two signals the

Mr. Wilson.

time was noted by a chronograph registering the time to one-tenth of a second, and the number of revolutions of the turbine was counted. The water inside the tank was the quantity which flowed through the pump during the time registered. If the tank was a small one he found, by repeating the operation say four times, and averaging them, that reliable results were obtained in ten or twelve seconds as the time of each operation. When operating with large pumps the trough was arranged with a flexible end.

Since the outlet-nozzles of the "Thornycroft" boat were above the water, this method of measuring the quantity of water flowing through them in a given time might readily be carried out in that boat. The small plate adopted by the Author would in his opinion register less pressures than actually existed by causing reactive pressures on the boat, inasmuch as the water after flowing round the curved outlet-pipes would not be in compact streams; this was proved by the fact that the pressures on the plate varied in intensity in different sections of the stream. Water, when it issued from a short curved pipe, spread laterally in a greater degree than when it flowed out of a straight pipe.

The question now arose, How was it possible that the reactive pressures tending to propel the boat, as calculated by the Author from the pressures of the water on the plate, could be doubtful, inasmuch as those reactive pressures were practically equal in intensity to the pull registered by the dynamometer at the stern of the boat? When the boat was moored and consequently at rest, the water flowing through the inlet-pipe caused back-pressure to the boat. The inlet-water flowed in an oblique direction mainly from forward to aft of the boat, therefore the inlet-water was partially deviated against the curved inlet-pipe into the pump, and such force of deviation caused back-pressure to the boat. The reactive pressures of the water issuing out of the discharge-pipes must evidently be equal in intensity to the pull registered by the dynamometer, plus the back-pressure to the boat caused by the force in deviating the water flowing through the curved inlet-pipe. If the precise direction in which all the atoms of the water entered the inlet-pipe and the angle at which the same were deviated were known, and if also the quantity of water flowing in a given time was ascertained, the intensity of the back-pressure to the boat could be calculated; but the shape, and the oblique position which the pump and inlet-pipe occupied in the boat were such that these facts could not be ascertained; nevertheless, the intensity of the back-pressure on the boat was considerable.

Mr. Wilson. Respecting the centrifugal pump, the Author calculated that at two hundred and ninety-five revolutions of the pump-wheel or turbine per minute the velocity of the water discharged through the nozzles was 24 feet per second. The diameter of the turbine wheel was stated to be 2 feet 6 inches, equivalent to 7·85 feet circumference. The velocity of the periphery of the wheel would therefore be $\frac{7 \cdot 85 \times 295}{60} = 38 \cdot 6$ feet per second, as against the velocity of the discharged water, namely, 24 feet per second. This showed an unusual slip for a centrifugal pump, which must cause an unusual loss of power. Again, according to the Author's calculations, the velocity of the water discharged from H.M.S. "Waterwitch" was 29 feet per second; and as the pump-wheel was 14 feet in diameter, and the revolutions forty per minute, therefore $\frac{14 \times 3 \cdot 1416 \times 40}{60} = 29 \cdot 3$ feet per second, the velocity of the periphery of the wheel; and, therefore, in this case there was practically no slip. These figures would seem to show that the "Thornycroft" centrifugal pump was inferior to the pump fitted to H.M.S "Waterwitch." He was of opinion, however, that such was not the case. The "Thornycroft" pump was, he believed, well designed for its purpose; but the pump had not yet been credited with its actual duty from two causes. One was that the weight of water passing through the pump and discharge-pipes at two hundred and ninety-five revolutions was greater than 1,426 lbs.; and the other that the water flowing through the suction-pipe was not sufficient to maintain a full volume through the pump and discharge-pipes when the pump was run at high speeds. The relative area of the suction- to the discharge-pipes in the "Thornycroft" pump was 1·52 to 0·951 square foot or 1·6 to 1. In the "Waterwitch" pump the sectional area of the inlet was 28·25 square feet against 6·28 square feet sectional area of discharge, equivalent to 4·5 to 1. In his experiments, as to the volume of water passing through pumps, he obtained augmented percentages of useful effect in shaping and arranging the size of the suction-pipe to the required speed of the pump-wheel.

The nozzles of the "Thornycroft" pump were not properly shaped to obtain the full effect of the reactive pressures of the water issuing from them in propelling the boat. To secure the full reactive pressure for the boat, straight pieces as tangents to the curve in lengths from one and a half to twice the diameter, should be joined to the end of the curved discharge-pipes. Such straight pieces might not be so convenient for reversing the

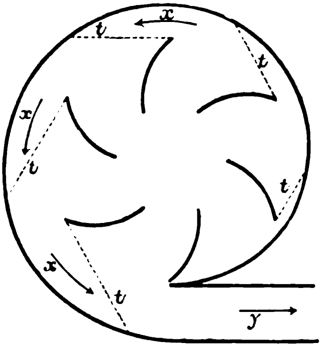
Mr. Wi

nozzles, but their absence prevented the full reactive pressures from being utilized.

Messrs. Thornycroft and Co. had succeeded in propelling their boat at 12·6 knots per hour, the highest speed hitherto attained by hydraulic propulsion. But the "Thornycroft" screw-boat, by the expenditure of an equivalent indicated HP., had attained a speed of 17·3 knots per hour. It was important to determine precisely the reactive pressures of the water issuing through the discharge-nozzles, inasmuch as those pressures constituted the basis for calculating what became of the indicated HP. of the engines, and where the loss of useful effect took place. He thought those reactive pressures could only be ascertained with certainty by measuring the volume of water discharged through the nozzles during a known interval of time. When the loss in useful effect had been more precisely ascertained, the same would be found to depend on the shape of the discharge- and inlet-nozzles, and particularly the position and direction in which the water was admitted, notwithstanding that some authorities thought that these points were immaterial. The main loss in useful effect, as the Author remarked, would be shown to exist in the centrifugal pumps. Those pumps gave out a fair percentage of useful effect when used for raising water to moderate lifts; but, when used for propelling purposes, wherever they had been tried the percentage of useful effect had been low. The difference in the percentage of useful work in a centrifugal pump for lifting water and propelling a boat was mainly owing to the difference of velocity, in the two cases in which the water was forced through the pump and discharge-pipes. In lifting water the speed of the pump-wheel was regulated to overcome the lift, and to discharge the water at a uniform velocity of from 7 to 9 feet per second, in order to obtain an economical result. On the other hand, for propelling purposes, the water was forced through the pump and discharge-pipe at a high velocity. In the "Thornycroft" boat, the Author calculated that, at a speed of 12·6 knots, the velocity of the water through the discharge-pipes was 37¼ feet per second, which was about five times higher than the velocity of the discharged water generally adopted when pumps were used for lifting water. The action of a centrifugal pump consisted in the water passing through the pump being thrown from the tips of the disk vanes in tangents to the periphery of the disk across the water flowing round the channel between the periphery of the disk and pump-casing. That action caused concussions of the atoms of the water, resulting in an important percentage of the power being converted into the

Mr. Wilson. mechanical equivalent of heat. This would be seen in Fig. 15, which was a skeleton section through a centrifugal pump:—

FIG. 15.



t . . *t* represented the directions in which the water was thrown from the tips of the disk vanes across the channel around which water was flowing in the line of the arrows *x*, *x*, and through the discharge-pipe *y*. The higher the velocity at which the water left the tips of the vanes, and also flowed round the channel, the greater would be the magnitude of the concussions of the atoms of the water.

For the purpose of obtaining a more economical apparatus in forcing water at high velocities, after carrying out numerous experiments he had designed a hydraulic wheel, the principle of which was shown in Figs. 16 and 17 (p. 77), which were sections through the apparatus.

The wheel (shown by the thick black lines), similar in shape to a grooved pulley or sheave of a block, rotated around the casing *m* . . . *m*, on the shaft *g*, which passed through the stuffing-boxes. A block K extending from Y to Y′, shaped in a suitable manner, fitted within and close to, but did not touch, the cavity of the wheel. The passage or channel for the water, extending from F to G round the casing, was of a uniform section *o*. Supposing the wheel to be rotated in the direction of the arrow *s*, the action of the cavity of the wheel forced the water around the casing in that direction, and although the wheel revolved freely over the block K, no water could pass; consequently, the water entering the suction-pipe F, was forced by the action of the cavity of the wheel round the casing, and discharged through the pipe G in a constant, uniform, and compact stream, the same in section as the cavity at *o*.

Mr. Wilson.

If the wheel was caused to rotate in a contrary direction to that of the arrow *s*, then the water entered the suction-pipe G and was discharged through the pipe F in a constant and uniform volume. The cavity of the wheel was turned and made as smooth as possible. The main principle of the action of the cavity of the wheel in forcing the water (which was not surface friction) was difficult to explain except by means of a model.

Fig. 16.

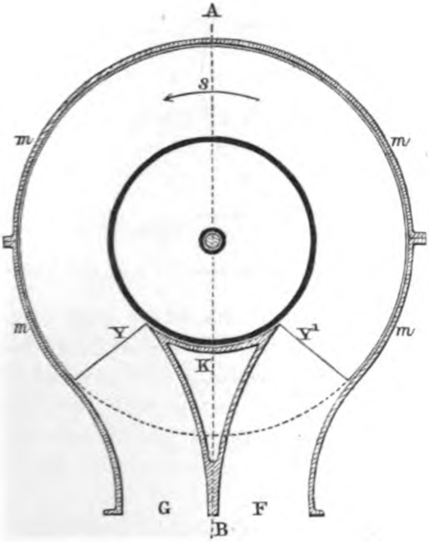
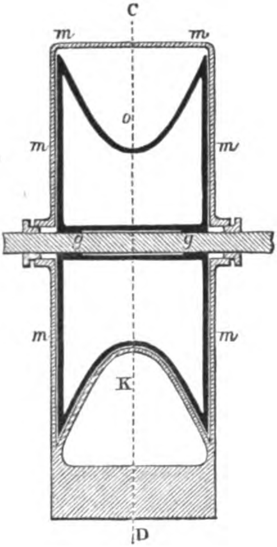


Fig. 17.



In order to obtain the volume of water necessary for propelling large vessels, without having to resort to a wheel with one large cavity and enlarging the diameter of the wheel, he had adopted a wheel with several cavities fixed on a single shaft.

Mr. Barnaby.

Mr. S. W. Barnaby, in reply to the correspondence, questioned the accuracy of Mr. Wilson's statement that, when the boat was moored and pulling against a dynamometer, the reaction of the discharge nozzles was balanced by the pull registered by the dynamometer plus a backward reaction caused by the deviation of the inflowing water in the curved inlet pipe. The sternward pressure upon the boat, caused by the change of direction of the inflowing water, was exactly balanced by the forward pressure of the outside water upon the hull of the vessel, caused by the relief of pressure at the inlet. Suppose the inlet to be stopped up, the statical pressures upon the hull would be balanced. If then

Mr. Barnaby. the inlet was unstopped, and water allowed to flow in, there would be a momentary unbalanced pressure driving the boat forward, which was immediately balanced by the sternward pressure exerted by the inflowing water as it was deflected upwards by the pipe, and there would be no resultant effect.

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4 March, 1884.

Sir J. W. BAZALGETTE, C.B., President,
in the Chair.

The following Associate Members have been transferred to the class of

Members.

Edward Golding Barton.
Mark William Carr, Jun.
George Edward Moore.
Richard James Harris Saunders.
James Stuart Swallow.
William Thomson.

The following Candidates have been admitted as

Students.

Léon Harry Barker.
Hugh Walter Belcher.
Arthur Coleman.
Alfred Howe Collinson.
John Rowland Crook.
Frederick William Cross.
John Richard Davison.
Frederic James Edge.
Andrew Forbes.
George Leopold Gregson.
Arthur Harnett.
George Hilder Libbis.
Thomas Lodwick Miller.
Fred Platt.
Hans Wessel Poppe.
Peter Augustus Ransom, B.A.
Frank Walter Scott, Jun.
George Thomas Sibbering.
John Bunting Simpson.
Charles Gordon Stuart.

The following Candidates were balloted for and duly elected as

Honorary Members.

The Right Hon. Lord Bramwell.
Sir William Robert Grove, M.A., D.C.L., LL.D.

Members.

Ralph Peacock.
William Willcox.
John Edward Wolfe.
Thomas William Worsdell.
James Walker Wright.
William Wright.

Associate Members.

- ARTHUR ROBERT BEYNON, Stud. Inst. C.E.
- JAMES JOHNSTONE BOURNE, Stud. Inst. C.E.
- CHARLES ERNEST BRUGES, Stud. Inst. C.E.
- ROBERT COPE HARDY DAVISON, Stud. Inst. C.E.
- JOHN DEWRANCE, Stud. Inst. C.E.
- LEONARD GILL.
- CHARLES FLEMING HARGREAVES.
- CHARLES HILTON HINGESTON, Jun., Stud. Inst. C.E.
- WILLIAM TINDAL JENNINGS.
- CHARLES ARTHUR LOVEGROVE, Stud. Inst. C.E.
- GEORGE CROYDON MARKS, Stud. Inst. C.E.
- ROBERT JOHN PRESTON.
- JOHN HENRY RILEY.
- WILLIAM PETTIT WARD, Stud. Inst. C.E.
- HERBERT FRANCIS WARING, Stud. Inst. C.E.
- LARS SEVERIN ZACHARIASEN.

Associates.

- JAMES ANDREWS.
- JOHN BURN-MURDOCH, *Lieut.* R.E.

The discussion upon the Paper on "Hydraulic Propulsion," by Mr. Sydney W. Barnaby, occupied the whole of the Meeting.

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11 March, 1884.

SIR J. W. BAZALGETTE, C.B., President,
in the Chair.

The discussion upon the Paper on "Hydraulic Propulsion," by Mr. Sydney W. Barnaby, occupied the whole of the Meeting.

18 March, 1884.

Sir J. W. BAZALGETTE, C.B., President,
in the Chair.

(*Paper No. 1958.*)

"Wire-Gun Construction."

By JAMES ATKINSON LONGRIDGE, M. Inst. C.E.

IN the year 1855 the Author submitted to the Ordnance Department of that day proposals for the construction of guns by steel wire coiled upon an inner tube. These proposals were based upon experiments previously made by him, which were generally described in the Paper read before this Institution in February 1860.[1] In the same year, the Author constructed the 3-inch wire-gun, which was first fired at Southport in that year, and which was exhibited in this Institution in 1879, when the Author's second Paper was read and discussed.[2] At various times between 1860 and 1879 the Author in vain attempted to obtain an official trial for his wire-gun system. Within the last four or five years other parties, amongst whom the first was probably Captain Schultz, of the École Polytechnique, took the matter up, and Sir William Armstrong soon after did the same. In August 1882 the Author was asked by the Secretary of State for War to give evidence before the Ordnance Committee on the "general question of gun-construction, as to metal, construction, breech-closing arrangements, form and size of chamber, obturation," &c.; but that body has not thought it desirable to do more than put a few general questions in writing which had no very important bearing on the matter.

Before entering on the specific subject of this Paper, it may be well to make a few further remarks upon the present system of hooped-gun construction, as referred to in some papers which have been received by the Institution from the Ordnance Department, U.S.A.[3] These papers are mostly translations from Virgile,

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[1] Minutes of Proceedings Inst. C.E., vol. xix., p. 283. [2] *Ibid.*, vol. lvi. p. 171.

[3] Notes on the Construction of Ordnance, Nos. 1–23. Washington, July, 1882—June 1883.

Colonel of the French Marine Artillery, Rosset, Director of the Turin Arsenal, and Clavarino, Captain of the Italian Artillery. The Author has also carefully examined Colonel Virgile's treatise on the Resistance of Metallic Tubes, simple or compound, published in Paris in 1874.

These documents are valuable and interesting, and show the great importance attached to the mathematical treatment of the subject of gun-construction by the French and Italian Governments. Their translation and publication by the Ordnance Department at Washington evinces a like solicitude on the part of the United States Government. The investigations of Virgile and Clavarino, who deal chiefly with the theory of the subject, although conducted in a somewhat different method, bear out exactly the conclusions and formulas in the Author's first Paper.

The fundamental formula, on which everything else depends, was independently arrived at by Dr. Hart,[1] of Dublin, and by the late Professor Rankine, and it is in accordance with the principle laid down by the eminent French mathematician and physicist, Lamé; but it differs materially from the formula of Professor Barlow.[1]

Although, as stated, the fundamental formulas of the various investigators arrive at the same result as was given in the Paper of 1860, there are one or two remarks which it is necessary to make respecting their application. The first is, that some modification must be effected in certain cases, by introducing into the results the effect of what are known as Wertheim's coefficients. This was brought before the Institution by Dr. Hopkinson in the discussion on the Author's Paper of 1879.[2] It is also fully dealt with by Virgile in his treatise before mentioned, and upon it the following remarks may be made.

When a bar of metal, length l, is subjected to the tension of a force f, the extension within the limits of elasticity is $l\frac{f}{k}$, where k is the modulus of elasticity. Now, if at the same time the bar is subjected to forces $f_{\prime}$ and $f_{\prime\prime}$ laterally, it may be proved, as is done by Virgile, that the extension will be no longer $l\frac{f}{k}$, but

$$l\left\{\frac{f}{k}+\frac{f_{\prime}}{\alpha k}+\frac{f_{\prime\prime}}{\beta k}\right\},$$

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[1] Minutes of Proceedings Inst. C.E., vol. lvi., p. 173.

[2] *Ibid.*, p. 212.

assuming that k has the same value for tension as for compression, and this assumption is either absolutely or very approximately true for such metals as are used in gun-construction.

From analytical considerations Poisson arrived at the values $a = 4, \beta = 2$; but Virgile shows that these estimates are incorrect, and arrives at the conclusion that a and β are each equal to 3, and this agrees with the result arrived at by Wertheim after a long series of careful experiments. Consequently, it may be taken as a fact that the actual extension of the bar is

$$l\left\{\frac{f}{k}+\frac{f_{,}}{3\,k}+\frac{f_{,,}}{3\,k}\right\}.$$

Now, in the case of a hooped gun, taking l as the length of a filament of indefinitely small thickness, f the circumferential strain, $f_{,}$ the normal pressure, or actual compression, and $f_{,,}$ the strain in the direction of the axis of the bore, the actual extension will not be $l\frac{f}{k}$, but

$$l\left\{\frac{f}{k}+\frac{f_{,}}{3\,k}+\frac{f_{,,}}{3\,k}\right\},$$

and the question is, what modification does this involve in the general formula for tension?

In guns constructed according to the Author's principle $f_{,,}$ would not exist, and in guns as ordinarily constructed it is shown by Virgile that its effect is so insignificant, as affecting the extension, that it may be neglected. It is therefore only necessary to deal with the modifying effect of the radial compression $f_{,}$ and the effect of this upon the general formula is fully shown by Virgile. It is not of serious importance in any case. Dr. Hopkinson calculated that in the 9-inch gun it would increase the tensile strain of the inner surface of the steel tube from 16·97 to 18·2 tons, and would decrease that of the iron hoop from 14·45 to about 14 tons per square inch. It may, however, be shown that these variations are due entirely to the fact, that the tube and the hoop have not the same modulus of elasticity. When the moduli of elasticity are the same, the normal or radial force has no effect upon the tension. Consequently, in a gun entirely of one material, with the same modulus, the radial force $f_{,}$ may be omitted in the calculation. But in guns built up of hoops having different moduli from the tube, their forces may be taken into account, although their effect is comparatively small.

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In wire-guns, as will be shown hereafter, the effects of the normal forces in determining the proper tension for laying-on the wire is very important, and must be considered; but here again the effect disappears if the modulus of the wire is the same as that of the tube.

The next remark on the work of Virgile is, that he has come to a conclusion respecting the maximum strength it is possible to give to a built-up gun which the Author cannot agree to. Virgile maintains that if a gun be so constructed, that whilst an internal powder-pressure raises the tension of the inner surface of the tube to the elastic limit of the material, yet in the same gun, when the internal pressure is removed the same inner surface is compressed to the elastic limit of compression, this internal powder-pressure is the greatest admissible for any gun of this material, however perfectly constructed.

As regards the tensile maximum, the Author is of the same opinion as Virgile, that it is inadmissible to strain the material beyond the elastic limit; but as regards compression, he is quite certain that the elastic limit may be safely exceeded, and that to a very large extent. His own experience has left no doubt upon his mind regarding this. In the gun he exhibited in 1879, the cast-iron tube, which was 3 inches in diameter, was reduced by the coiling on of the wire by $\frac{1}{25}$ inch, whilst the elastic limit of the material would be reached with a reduction of $\frac{1}{400}$ inch.

It is impossible to say to what extent the material on the inner surface of a tube may be compressed without injury. The ordinary experiments on crushing, by a force applied to one side only of a prism, have no bearing on the question. The effect in a tube seems to be to condense the material, and thus to strengthen it. Clavarino discusses this subject at considerable length, and gives it as his opinion, fully confirmed by the results of experiments, that a compression of three times the elastic limit is perfectly admissible.

In the development of his formula, Virgile treats only of the case of calculating the dimensions of a gun to resist a given internal pressure, whilst at the same time its parts shall not be strained beyond a given limit. Clavarino goes much further than this, and gives formulas showing the resulting strains when the shrinkages applied are not in accordance with theory, and this was the direction which the Author took in his former Papers, although the determination of the proper shrinkage was therein contained. Both Virgile and Clavarino insist upon the proper determination of the shrinkage, as the vital element in the problem of hooped gun-construction; and in this, whilst agreeing with the views of the Author, they differ from those expressed by Sir William Armstrong and the Woolwich authorities, who appear to think that although a gun may be built up with shrinkages differing in excess from the correct shrinkages, it will as it were shake itself into a proper condition by repeated firing. It is just possible that in certain cases it may do so, but it will be by chance only.

To dwell for a moment on this point, if a hoop is put on another with an amount of shrinkage above what calculation indicates, the effect, when the internal pressure is applied, is to strain the inner surface of the super-imposed hoop beyond the elastic limit of the material. This gives rise to what is known as permanent set. Now if it should so happen that the excess strain causes a permanent set just equal to the original excess shrinkage, it is clear that the gun has been put right, that is to say, into the condition under which it ought to have been built. Subsequent firing will not alter these conditions, for the simple reason that the strain of explosion no longer exceeds the elastic limit. But it is possible, and in fact frequently happens, that the permanent set caused by excess of strain greatly exceeds the excess shrinkage. In this case the gun is reduced to the condition of deficient shrinkage, and it may be of no shrinkage at all, and it will inevitably fail.

In guns with steel tubes the failure generally is first indicated by the cracking or splitting of the tubes, but it may likewise so loosen the hold of the hoops and trunnion-ring as to cause the failure of the gun by tearing asunder.

An instance of this may be specified in the failure of the 6·3-inch gun, on board H.M.S. "Daring," on the 22nd of February, 1883, resulting in the death of two men and the wounding of three others during target-practice. The Author, being anxious to investigate this failure, has obtained such details of the 6·3-inch guns in the service as have enabled him to do so.

There are three types, or "natures" as they are called officially, of 6·3-inch guns in the service. One of them has a steel tube, with a wrought-iron hoop or jacket on the Fraser system. The other two are entirely of wrought-iron. The first, which was also the first type, consisted of a wrought-iron tube 2·1 inches thick, then a wrought-iron breech-hoop 3·25 inches thick, and outside of this another wrought-iron hoop 2 inches thick, thus making a total thickness of 7·35 inches over the powder-chamber. This gun is called Mark I., and was the first type of the 6·3-inch guns made at Woolwich. Subsequently the type was altered to what is called Mark II., and this consisted of a wrought-iron tube 2·100 inches thick, over which was an iron hoop 3·250 inches thick, and on this again another iron hoop 3·875 inches thick, making a total thickness over the powder-chamber of 8·225 inches. It was one of these two types which burst on board the "Daring," but not being able to ascertain which of the two, the Author has investigated each case separately. The internal powder-pressure, with the powder used in these guns, is about 24 tons per square inch. In the case of Mark II., this would give the following strains at the inner surface :—

	Tons.	
Tube	12·050	per square inch.
First hoop	11·871	
Second "	16·574	

Now as the elastic limit of the iron does not exceed 11 tons per square inch, it is evident that permanent set would take place at each surface of contact. This would be of no consequence at the inner surface of the tube, and would be insignificant at the junction of the surface of the tube and first hoop, but between the first and second hoop it would not only annihilate the shrinkage but would expand the outer hoop nearly $\frac{2}{100}$ inch beyond the hoop in which it was placed. Consequently the gun is now reduced to the condition of a single-hooped gun.

A second firing would give rise to strains of 23 and 20 tons per square inch at the surfaces of the tube and hoop respectively, and would cause a permanent set, which would make the hoop nearly $\frac{1}{100}$ inch larger than the tube. By the third firing the strain on the inner surface of the tube would be raised to about 50 tons per square inch, and the gun would burst.

The result with the gun Mark I. would be very similar.

Now it will no doubt be said that these guns did fire many more than three rounds. That is true, but the reason is not far to seek. In the calculation it has been assumed that after separation of any two tubes by permanent set, the outer tube would no longer be of any service, but this is not quite so. The second fire closes up the space between the hoops caused by the first, and then acts upon the outer hoop, again giving rise to a new permanent set, and this process goes on by each fire; but the ultimate result is that the utility of the hoops is successively destroyed, and the inner tube is left to do nearly the whole of the work.

The process may be more or less long, but the final destruction is inevitable.

In the case of the "Daring" gun, the Author thinks that probably the process never reached the actual bursting of the gun, but that the continuous expansion of the hoops by permanent set so loosened their hold upon each other, that the longitudinal strain tore the gun asunder at some point a little behind the trunnions. However this may be, it is clear to his mind that these guns are utterly unsafe, and that they ought immediately to be withdrawn from the service. It was stated in the House of Commons, by the Secretary to the Admiralty, that these guns were now ordered to be fired only with reduced charges, and that all guns of this calibre were now constructed with steel tubes. As regards reduced charges, it is probable that the guns will be safe so long as the powder-pressures are kept below 10 tons per square inch. The effect of this on the efficiency of the guns need not be stated.

The steel-tubed 6·3-inch guns as now constructed, Mark III., are probably safe guns with a powder-pressure of 13 tons per square inch, but beyond this they are unreliable.

In treating of the subject of permanent set and of the effect of strains exceeding the elastic limit, there are some very interesting remarks in two of the American "Notes" above mentioned. Note No. II., dated 20th September, 1882, is a translation of a portion of a memoir by Colonel Rosset, Director of the Turin Arsenal, on the resistance of metals used for ordnance.

Colonel Rosset states, that up to a certain point the elongations were proportional to the stresses, but beyond that, they increased rapidly, and in a varying proportion, and that when in excess of the elastic limit, permanent elongations remain after the strains are removed. When the bar is again submitted to tension it is found that the permanent elongations do not begin so soon as before, and that consequently the elastic limit of the material has been increased, though the modulus of elasticity remains practically the same. This extension of the elastic limit is called by Colonel Rosset "Special Elasticity," and it is desirable to ascertain to what extent it can be practically carried.

As regards the use of hoops in gun-construction, Rosset has proved that only where the permanent elongation results from mechanical force does the extension of the elastic limit take place, and that there is no such extension when the elongation is the effect of contraction consequent upon cooling a bar or hoop expanded by heat. From which it follows that this peculiar quality does not influence the construction of hooped-guns as generally practised, but has some influence on their behaviour under fire, and this has been taken into account by the Author in his investigations into the 6·3-inch gun of the "Daring."

In concluding these preliminary remarks the Author wishes to state that after the most careful consideration, he is forced to the conclusion, that the construction of hooped-guns is beset with enormous practical difficulties, arising mainly from two causes; first, the extreme accuracy of workmanship required to ensure the correct shrinkages, and secondly, the varieties in the physical properties of the great masses of metal required for guns of large calibre. He would further desire to place once more on record his opinion that the armament of this country, as regards Heavy Ordnance, is altogether unreliable, and may lead not only to such lamentable casualties as those of the "Thunderer" and the "Daring," but to national disasters of the gravest character.

To turn now to the more special subject of this Paper, wire-guns. There is a good deal of misconception regarding the use of wire in gun-construction. It is generally regarded as merely the substitution of a very strong material in its strongest form. Steel wire can be obtained with a tensile force of fully 100 tons per square inch, and it is thought very desirable to substitute it for ordinary steel, which will not bear one-half that strain. It is further thought that, by winding it on a tube, the tube will be greatly strengthened by a process at once more convenient and more economical than by hooping.

Now all this is true; but the essential feature of Wire-Gun Construction is very different. It consists in the facility which it affords of bringing the whole body of the gun into the proper state of varying initial strain, so that when the powder-pressure acts, every portion of the structure may be equally strained. Consequently, the most important thing is, to determine the proper tension with which to lay on the wire. By some it is considered sufficient to lay it on with just so much tension as will ensure close contact, that is to say, with practically no tension at all. This is the plan recommended by Dr. Woodbridge, who has, however, proposed a great variety of modes of construction, viz.:—

1. Cast-iron gun strengthened with wire, wound with a tension of about 22 tons per square inch, and then immersed in a bath of solder.

2. A steel gun, composed of an inner steel tube overlaid with longitudinal steel bars, and then wound round with steel wire, under high tension, and finally soldered.

3. A steel tube wrapped with steel wire and brazed.

4. A similarly constructed gun, but with the addition of a reinforce of hard-drawn steel wire wound over the chamber.

The third system is, as far as the Author can learn, the only one which Dr. Woodbridge has actually carried into practice.

A 10-inch gun was thus constructed for the United States Government at the Frankford arsenal in 1873. The wire was $\frac{3}{10}$ inch square; it was laid on with just sufficient tension to ensure close contact, and twenty-nine wires were wound on at a time, forming a band of about 9 inches wide, and consequently with a considerable obliquity to the axis of the gun. The gun was then placed vertically in a mould, heated to the temperature of melting bronze, and bronze was poured in and kept in a state of fusion, until it was supposed the process of brazing was complete.

This gun did not prove a success, nor was it possible it should. It was altogether wrong, both in principle and in practice, and it is only mentioned here because it has often been asserted, that the wire construction has been tried by the United States Government, and has failed.

By some it is held that the tension of laying-on the wire is not a matter of great importance, provided it has a pretty good strain on it, and that the best way of laying it on is to keep the tension uniform throughout.

The Author is not aware of the principles on which Captain Schultz and Sir William Armstrong and Co. regulate the tension of laying-on, beyond the fact that, from a description in the "Report of the Board on Heavy Ordnance and Projectiles." Senate. Document 178, p. 107 (1882), describing Captain Schultz's 34-centimetre (13·39-inch) gun, it appears that the wire was 0·118 inch in diameter, and was laid on with a uniform tension of 1,100 lbs., or about 45 tons per square inch. The Author will return to this gun further on.

The question of the amount of tension of laying-on is one of mathematical physics, and is amenable to mathematical treatment. There are many variables in the problem, such as the following:—

1. The ratios of the inner and outer radii of the tube or core, and of the wire coil.

2. The moduli of elasticity of the materials. The elastic limits of the same.

3. The powder-pressure to be resisted.

4. The permissible strain, under this pressure, of the material of the core and of the wire.

5. The permissible compression of the material of the core when there is no internal pressure.

It is impossible, in a Paper like this, to give any idea of the analysis of this problem, but the Author hopes, by means of diagrams, to convey some useful information on the chief results he has obtained.

The investigations are somewhat puzzling to any one who has not given steady and persevering consideration to the problem; and the Author has again to acknowledge the valuable assistance he has received from Mr. Charles Henry Brooks, F.R.A.S., in the mathematical part of the subject.

Nearly thirty years ago Mr. Brooks and the Author established a formula for the laying-on tension, with the condition that the wire coil should be uniformly strained when under fire. They then determined the strains on the tube or core under fire; and further, the strains on both core and coil when the internal pressure was removed.

Fig. 1 shows the result of this formula as applied to a 12-inch gun, with a tube or core 2 inches thick, and with 8·91 inches of steel wire.[1] The core was assumed to have a modulus of elasticity of 4,500 tons, and the wire of 22,000 tons. The internal powder-force was assumed to be 30 tons per square inch. The dotted - - - - - curve represents the tensions of laying on, showing 31·747 tons per square inch for the first wire, and 8·574 tons for the last. The curves marked thus — - - — - - — - - represent the state of the gun when there is no internal pressure, which is called the state at rest.

For the coil this shows—

Compression of first wire . . .	11·237 tons per square inch.
Tension of last wire	8·574 „ „ „

For the core—

Compression of inner surface	11·257 tons.
„ outer „	8·794 „

---

[1] In this and all the following figures the strain under fire is denoted thus . . . . ————————
Laying-on tension is denoted thus . . . . . . — — — — — — —
The strain at rest „ „ . . . . . . —·—·—·—·—·—·—
K is the modulus of elasticity of the core.
C „ „ „ „ coil.

The full lines show the state under fire, viz.:—

Tension of coil uniform	20	tons.
„ core inner surface	5	„
„ „ outer surface	2·23	„ compres

Fig. 2 shows the state of the same gun, if a steel core of same modulus of elasticity as the wire is used instead of the modulus core of Fig. 1.

Fig. 1.

$\frac{K}{C} = \frac{4 \cdot 5}{22}$

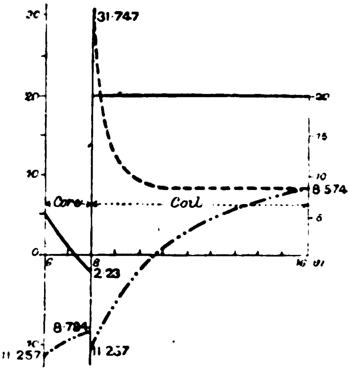
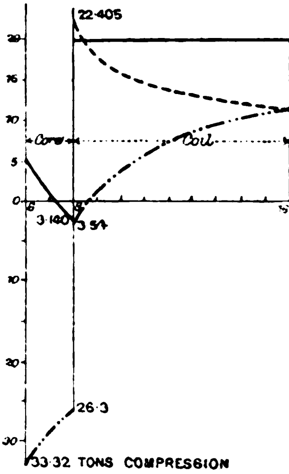


Fig. 2.

$\frac{K}{C} = 1$



It will at once be seen how greatly the strains are change this alteration of the modulus of the core.

The laying-on tensions are now—

First wire . . .	22·405	instead of 31·747	tons per square inch.
Last wire . . .	11·537	„ 8·574	„ „ „

The state of rest of coil is—

First wire, compression . . .	3·54	instead of 11·237	tons.
Last wire, tension	11·537	„ 8·574	„

And for the core at rest—

Compression of inner surface . .	33·321	instead of 11·257	tons.
„ outer surface . .	26·321	„ 8·794	„

From this it is evident that the hard steel core has to suppo large amount of the strain, which in the first case was borne the wire coil. This question will be dealt with more fully hereafter.

The point to which attention is now drawn is the important influence of the relative moduli of elasticity of the core and of the coil.

In the discussion on the Author's Paper of 1879, Dr. Hopkinson drew attention to the effect of the normal forces or radial compression on the strains in a hooped gun, but he showed that, in the particular case he then dealt with, the effect was not important. This arose from the fact that, in that case, the moduli of elasticity of the cores and of the hoops differed only about 50 per cent.; when the difference is greater the modification of tension is also greater, but when the moduli are the same the effect vanishes.

The same is the case with the wire coil, and it was therefore necessary to re-investigate the whole matter as regards wire coiled on a core of a different modulus.

The result of this investigation shows that the introduction of the normal forces very greatly modifies the laying-on tensions, as well as the states of the gun under fire and at rest, when the moduli of elasticity differ; but, as with hoops, this modification becomes null when the moduli are the same.

Figs. 3 and 4 illustrate this when compared with each other and with Figs. 1 and 2.

It may here be mentioned that Figs. 4 to 16 inclusive, are for a gun of the same dimensions, and with the same internal pressure.

The upper dotted curve in Fig. 3 shows the laying-on tension with a soft core,[1] when the normal forces are taken into account, as compared with Fig. 1, where these are omitted. It shows the following varieties:—

Laying-on tension of first wire . . .	2·870	instead of	31·747 tons.
„ „ last „ . . .	7·736	„	8·574 „

And on the coil at rest—

Compression of first wire	13·521	instead of	11·257 tons.
Tension of last wire	7·736	„	8·574 „

For the coil under fire—

Tension of wire uniform throughout 20 tons per square inch, the same as in Fig. 1.

---

[1] By "soft" is meant having a low modulus of elasticity.

For the core at rest—

Compression of inner surface	4·60	instead of	11·257 tons.
„ outer surface	3·59	„	8·794 „

Thus showing a great decrease of laying-on tension, a great decrease of the compression of the cores at rest, and an increase of the compression of the first wire of the coil at rest.

Fig. 4 is computed by the same formula as Fig. 3, making however, the modulus of the core equal to the modulus of the wire, and it is identical with Fig. 2, which was calculated from the original formula, thus showing, as before stated, that the effect of the normal forces vanishes when the moduli are the same. Fig. 3, as compared with Fig. 4, shows the great advantage of a soft core, as reducing both the laying-on tensions, and the compression of the core at rest; the play of the inner surface being 9·6 tons in Fig. 3, as against 38·321 tons in Fig. 4. It may, however, be remarked that in Fig. 4 the steel core, whilst under fire, is doing very little work, but, as will be shown hereafter, it may be made to take its full share by decreasing the amount of wire put on.

FIG. 3.

$\frac{K}{C} = \frac{4\cdot 5}{22}$

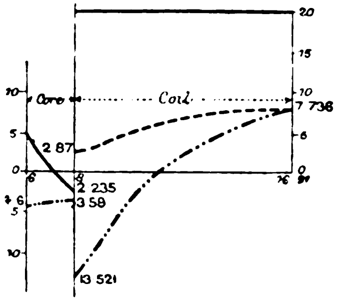
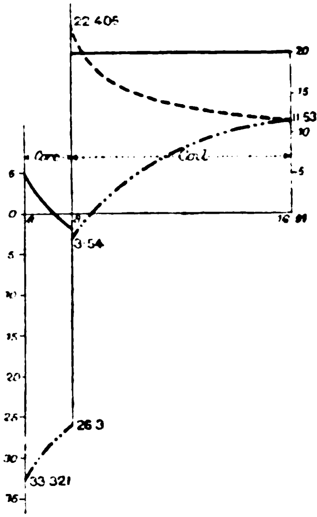


FIG. 4.

$\frac{K}{C} = 1$



In all these diagrams the calculations have been made for a uniform tension of the coil of 20 tons per square inch under fire, and a maximum tension of the inner surface of the core of 5 tons per square inch, with an internal powder-pressure of 30 tons per square inch. The formulas are, however, perfectly general, and are applicable to any dimension of gun, and any nature of material. In actual practice, certain of the variables have given values—for instance, the maximum internal powder-pressure, the limit of strain on the material of the coil and of the core, and the moduli of elasticity of the same. With these data, it is possible to determine the thickness of the core, the amount of wire to be put on, and the laying-on tensions.

On the other hand, having given the laying-on tensions, the dimensions of the gun, and the moduli of elasticity, it is possible to determine the strains on every part of the structure induced by any proposed internal pressure.

Uniform Tension of Laying-on Wire.

It has already been stated that by some it is thought the wire should be laid on with a uniform tension. Even if this were so, it would be necessary to ascertain what that tension should be.

The Author therefore, with Mr. Brooks' assistance, has investigated this problem, which is much more difficult than the other, and he will now endeavour to exhibit the results by a series of diagrams, all of which are computed for a gun of the same dimensions as given above, with the same internal pressure, and for the two cases, of the moduli of elasticity being the same in one case, and different in the other.

The first series of diagrams are those of the gun on which wire is laid with the following uniform tensions, the moduli of elasticity being in the ratio of 4·5 to 22, the same as in Figs. 1 and 2.

	Figures.	
Uniform tension of laying on .	5	4 tons per square inch.
	6	6 " " "
	7	10 " " "
	8	20 " " "

These diagrams show that, with a low tension of laying on, the inner coils are the most strained, whilst with the highest tension they are still in compression whilst under fire.

Comparing Fig. 5 with Fig. 8, the tensions are:—

First wire 24·67 tons tension as against 10·94 tons compression.
Last " 16·27 " " " 32·27 tension.
For the coil at rest 8·90 against 44·52 compression.
" core " 3·36 " 18·17 "
And for the core under fire 6·6 tension against 8·37 compression.

In all these diagrams, as also in Fig. 3, it is evident how v

FIG. 5.

$\frac{K}{C}=\frac{4\cdot5}{22}$

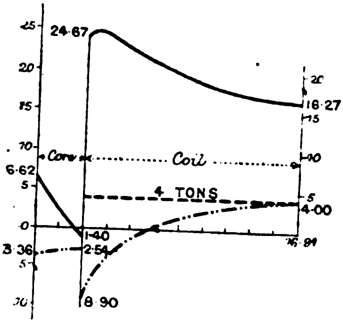


FIG. 6.

$\frac{K}{C}=\frac{4\cdot5}{22}$

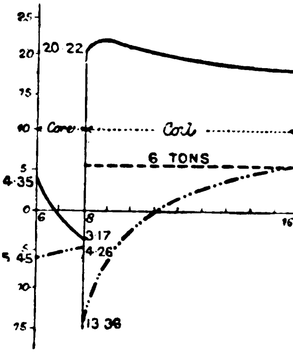


FIG. 7.

$\frac{K}{C}=\frac{4\cdot5}{22}$

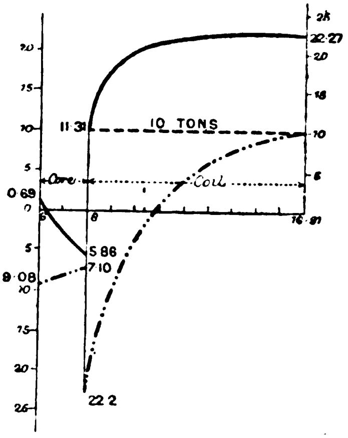
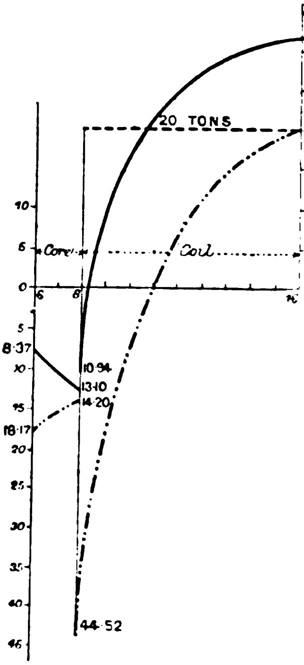


FIG. 8.

$\frac{K}{C}=\frac{4\cdot5}{22}$



little work is thrown upon the core, and this is entirely due to i low modulus of elasticity. It does little more than transmit the strains to the coil outside.

Another series of diagrams, Figs. 9 to 13, both inclusive, show the tensions in the same gun under precisely the same circumstances, excepting that the ratios of the moduli are increased from $\frac{4\cdot5}{22}$ to $\frac{9}{22}$.

They present the same features as the last series, but indicate that whilst the extremes of tension and of compression of the wire are reduced, the work thrown upon the core is considerably increased. The play upon the inner surface of the core, that is to say, the range between the extreme of tension under fire and of compression at rest, is just doubled.

Fig. 9.

$\frac{K}{C}=\frac{9}{22}$

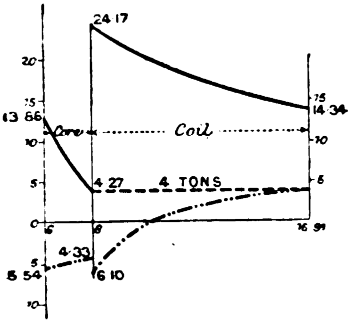


Fig. 10.

$\frac{K}{C}=\frac{9}{22}$

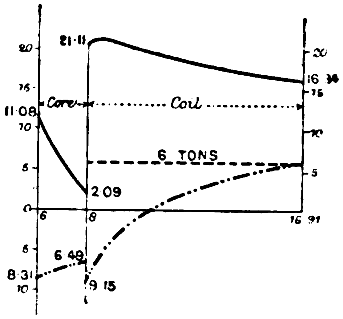


Fig. 11.

$\frac{K}{C}=\frac{9}{22}$

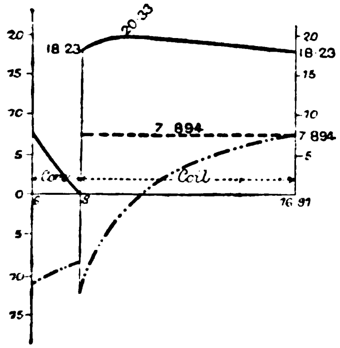
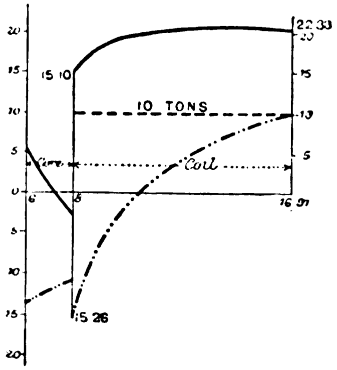


Fig. 12.

$\frac{K}{C}=\frac{9}{22}$



A further series of diagrams, Figs. 14, 15, and 16, have been calculated for the same gun with a core having the same modulus of elasticity as the wire, but in all other respects identical with the others.

Fig. 14 shows the strains with wire laid on with a uniform tension of 4 tons; Fig. 15 with 10 tons; and Fig. 16 with 20 tons.

Here again similar features are presented, viz., the reduction of strain on the coil, and the increase on the core. The play of the core is now increased to 38½ tons per square inch, whilst with the first series it was about 10 tons only.

FIG. 13.

$\frac{K}{C} = \frac{9}{22}$

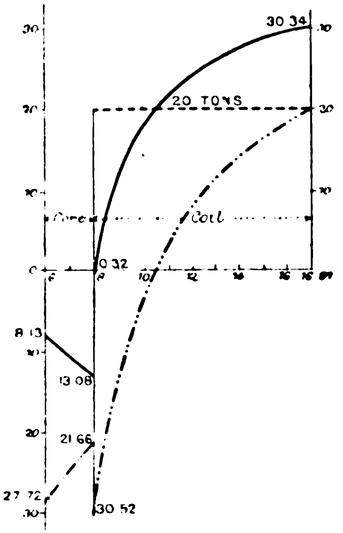
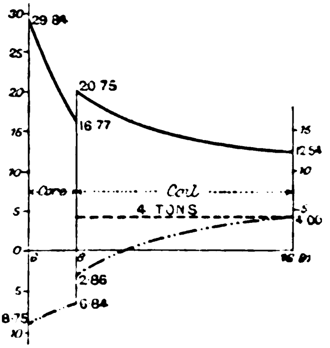


FIG. 14.

$\frac{K}{C} = 1$



These diagrams demonstrate conclusively the great danger incurred by a neglect of the right amount of tension in laying-on, and of ignoring the modulus of elasticity. For instance, Fig. 14 shows that with a steel core and steel wire laid on with a strain of 4 tons per square inch, the tension on the inside of the core under fire would reach 30 tons, whilst if the wire were laid on with 20 tons (Fig. 16), the inside of the core would be 5 tons in compression, and the outside wire 28·81 tons in tension, whilst at rest the inside of the core would be compressed to no less than 43·76 tons. Such tensions and compressions are far beyond the assumed elastic limit, and although this is of not much consequence as regards compression, it is of vital importance as regards tension, for then permanent set would take place, and materially alter the whole condition of the gun.

These diagrams further show, that for each particular gun there is a "particular" tension with which, if the wire be uniformly laid on, the strain of the first and last wires under fire will be the same. Take, for instance, Figs. 10 and 12. In Fig. 10 the tension of the first wire exceeds that of the last by 5·23 tons. In Fig. 12 that of the last wire exceeds that of the first by 5·16 tons. It is evident, therefore, that there must be some tension of laying on intermediate between 6 and 10 tons which will give equal tensions to both wires under fire. The formula enables this tension to be determined for any gun; and in the present case it is found to be 7·894 tons (Fig. 11).

FIG. 15.

$\frac{K}{C} = 1$

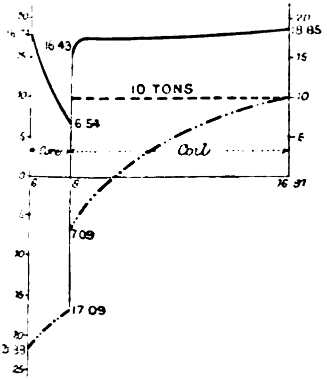
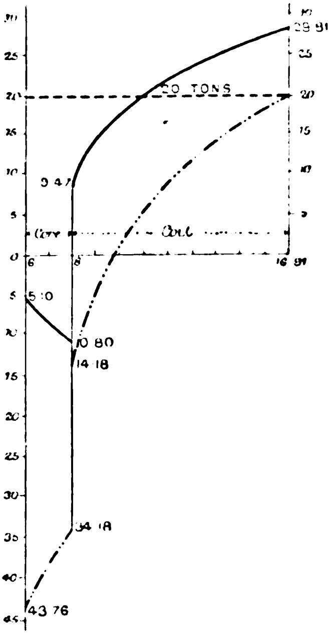


FIG. 16.

$\frac{K}{C} = 1$



On calculating with this tension of laying-on, it is found that the strain under fire is 18·23 tons for both the first and the last wire. But it does not follow, therefore, that the coil is uniformly strained throughout under fire. In fact, in this case the strain rises from

18·23 tons at the first wire to 20·33 tons on the wire 2 inches from it, and then falls again to 18·23 tons at the last wire. Although this "particular" tension of laying on is an approximation only to the solution of the problem of a uniform strain under fire, it would probably in some cases be sufficiently correct, and would no doubt be simpler for practical use.

Referring again to Figs. 2 and 4, it is obvious that the steel core gives hardly any resistance to the internal pressure. In fact, whilst the whole force to be resisted is 180 tons, the coil takes up 178·38 tons, and the core only 1·72 ton. The remedy for this is to diminish the thickness of the coil, or the tension under fire. If the coil be reduced from 8·91 to 7½ inches, Fig. 17 shows the resulting strains. Under fire, the coil is strained exactly as before, but the core has now an average strain of about 15 tons per square inch, and takes up 30 tons of the internal pressure instead of 1·72 ton.

After full consideration of the relative merits of laying-on with varying and uniform tensions, the Author is of opinion that though the former is preferable, yet the latter, from the greater simplicity of the operation, may perhaps be advantageously applied, provided the special tension for each individual gun be first ascertained by a correct formula; but it must be borne in mind that, except under these conditions, wire-gun making is but guess-work.

As an example of the error which may be made, the Author will briefly refer to Captain Schultz's 34-centimetre gun, as described in an extract contained in the Report of the United States Ordnance Board, above mentioned, for 1882.

The gun has a steel tube or core about 5 inches thick, on which is coiled a layer of about 4½ inches of steel wire. The exact dimensions are:—

	Inches.
Radius of inner surface of core	7·285
,, outer ,, ,,	12·195
,, ,, ,, coil	16·725

The wire used was 0·118 inch in diameter, and was laid on with a uniform strain of 1,100 lbs., or about 45 tons per square inch. This strain is about double the elastic limit, even allowing for the increase above referred to, as attendant on permanent set. It may therefore be taken that the wire was coiled on at its full elastic limit.

In order to examine the resistance of this gun, a limit must be fixed for the tension of the core under fire, and also for its compression at rest. These limits the Author has fixed at 20 tons per square inch in each case. He has further assumed that the modulus of elasticity of the steel tube was the same as of the wire. With these data it is found that the corresponding powder-pressure is 27·21 tons per square inch, and with this the core would be uninjured, but what would be the effect on the coil? In laying-on the wire was already strained to its elastic limit: what would be the effect of the internal powder-pressure?

This is shown by Fig. 18. The first wire would be strained to 28·14 tons, the last to 36·23 tons per square inch. In both cases a further permanent set of the wire would result, the tension

FIG. 17.

$\frac{K}{C} = 1$

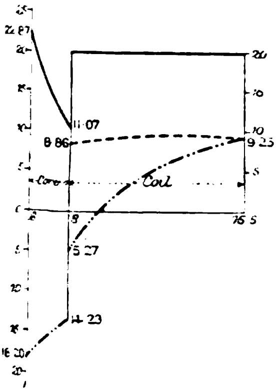
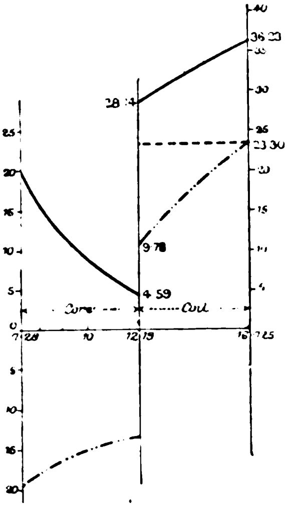


FIG. 18.

$\frac{K}{C} = 1$

$P = 27 \cdot 20$



of the coil would be permanently lowered, and the tension of the core in the next round increased, and sooner or later the gun would fail. Its original condition is at variance with the fundamental rule of ordnance construction, that no tensile strain should ever reach the elastic limit.

In order to make this gun safe, the maximum strain on the wire under fire should not exceed 20 tons, whilst the same limit may be taken for the steel core. With these limits, the dimensions of the gun remaining unaltered, the uniform tension of laying-on would be 10·70 tons, and the strains would be represented by Fig. 19. They would be: maximum strain on wire and on inner surface of core each 20 tons: maximum compression of core at rest, 9·02 tons. The maximum powder-pressure which the gun would bear with the above limitations would however be reduced to 19·87 tons per square inch.

The above illustrations will give some idea of the uses to which the Author's formula may be applied, but they extend much further; in fact they enable the proportions of a gun of given material to be designed to resist a given internal pressure; or, on the other hand, the resistance of any gun of which the dimensions and details of construction are given to be analysed.

The subject of Wire-Gun Construction is, however, not yet exhausted.

Clavarino has put forth an opinion, that the strength of a gun is limited not by the tension of the material, but by its extension. It amounts in fact to this, that the extension, say of a wire, by the joint action of the longitudinal and the lateral strains, fixes the limit of strain which the wire can resist. He says: "If the elongation (caused by the three forces) equals that which is produced at the limit of elasticity by simple traction, then the prism is on the point of undergoing a change of form, although the force acting in the direction of elongation may not equal the resistance at the elastic limit."

The inference he draws from this is, that the prism or wire cannot be safely subjected to a tensile force equal to its elastic limit.

Now the Author thinks this is altogether at variance with facts.

A wire of $\frac{1}{8}$-inch diameter may, it will be assumed, be safely strained without permanent elongation to 20 tons per square inch. Let now this wire be drawn down to half its diameter, it will bear at least the same strain per square inch, although it has undergone a very large change of form. Mere change of form does not necessarily imply reduction of strength; in fact, in the case of wire-drawing, it considerably increases it; and the action of the normal forces on a wire in a gun-coil is simply a case of wire-drawing. The material is no doubt elongated, and possibly condensed, but it is not therefore in any way weakened. The Author is accordingly of opinion that the tension may be safely carried to the elastic limit, although the extension may be increased beyond that of a free wire.

Although differing from Clavarino's views, the Author requested Mr. Brooks to investigate a formula showing the effect of treating the extension of the wire throughout the coil as constant under fire, instead of the tension. Now in dealing with this question it is found that in each gun there is a limit, below which it is impossible to reduce the tension of the last wire when under fire, the internal powder-force being given. For instance, with the gun to which all the foregoing diagrams from Fig. 1 to Fig. 16 refer, it is impossible on this hypothesis to reduce the tension of the last wire, when under fire, below 23·10 tons per square inch. With this tension the first wire would be laid on under tension zero. With any lower tension under fire, the first wire would have to be laid on in compression, which is practically impossible. Therefore, on this hypothesis, if it be imperative to reduce the tension of the last wire under fire to 20 tons, it will be necessary to alter the proportions of the gun, or to reduce the internal powder-pressure, and therefore the gun would be weaker.

FIG. 19.

$\frac{K}{C} = 1$

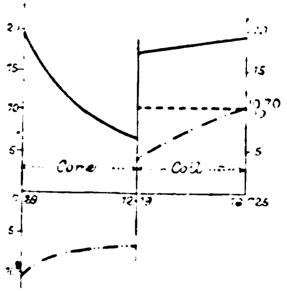


FIG. 20.

$\frac{K}{C} = \frac{4 \cdot 5}{22}$

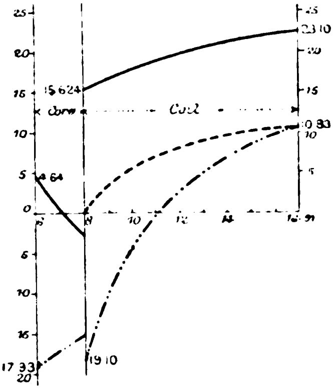


Fig. 20 represents a gun of the same proportion as the others, and with the same internal pressure, but with a tension of 23·10 tons on the last wire when under fire. It may be compared with Fig. 3 when the tension of coil under fire is uniform.

It will be seen that the tension of laying-on the first wire is zero, and of the last 10·82 tons per square inch. The tensions under fire are, first wire, 15·624; last wire, 23·10 tons. Of the inner surface of the core under fire, 4·60, and at rest, 17·93 tons compression: whereas with the constant tension under fire the compression of the core was only 4·6 tons.

Three objections have been raised to the wire-gun system, with regard to which the Author thinks it incumbent upon him once more to state his opinion.

The first is the assumed want of longitudinal strength. In his

Paper, in 1860, he described how this was to be provided for; in his second Paper he not only did so at more length, but exhibited a gun in which the provision had actually been made. It was, therefore, with no little surprise that he saw it stated in Colonel Maitland's Paper, read before the Society of Arts in February 1882, that in consequence of this difficulty the Author "who invented the system was compelled to abandon it."[1] Sir William Armstrong also, in his Presidential Address to this Institution on the 10th of January, 1882, stated that the Author had "suggested a form of gun designed with a view of obviating the objection of want of longitudinal strength, but I am not aware that his method of construction has been reduced to practice."[2] Not only had this been done by the Author, but it had been done with perfect success in the wire-guns constructed under Captain Schultz's supervision by the French Government. There is no difficulty in the matter.

Sir William Armstrong has adopted a plan of introducing longitudinal wires between the coils "in the proportion of one longitudinal layer to four circular layers. The longitudinals are secured to the trunnion ring at one end and to a breech ring at the other." This method does not commend itself to the Author's judgment. It is complicated; it interferes with the proper action of the coil; and it involves the practical difficulty of distributing the strain equally between a great number of independent masses of material.

The second objection is that dwelt upon by the late Sir William Siemens, in the discussion of February, 1879, viz. the alleged disturbances of the tensions by the heating of the gun consequent upon firing. To this the Author will only say, that there is no more difficulty in providing for the effect of heating in a gun than there is in providing for the changes of temperature to which a large iron bridge is subject.[3]

A third objection, also raised by Sir William Siemens, was the compressive action of the coil on the core when at rest. He said, "The Author, in all his calculations, spoke only of what would take place when the powder-pressure of 24 tons to the inch was applied; he did not say what would be the case when the shrinkage-pressure was applied in the first instance."[4]

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[1] Journal of the Society of Arts, vol. xxx., p. 335.

[2] Minutes of Proceedings Inst. C.E., vol. lxviii., p. 47.

[3] This question will be discussed at some length in the Author's forthcoming treatise on the "Application of Wire to the Construction of Ordnance." [This treatise has since been published, and a copy is in the library of the Inst. C.E.]

[4] Minutes of Proceedings Inst. C.E., vol. lvi., p. 208.

On the contrary, the diagrams then exhibited clearly showed the state of the gun both under fire and at rest. Sir William Siemens repeated, that "the shrinkage-pressure would undoubtedly have the effect of crushing or deforming the tube permanently. The tube would be no more able to resist such an amount of shrinkage-pressure than if it did not exist at all; and therefore such shrinkage as was contemplated by Mr. Longridge could not practically be applied."

In reply to this it is only necessary to point out, that the amount of shrinkage-pressure may be regulated at will, and the diagrams now exhibited show, in Fig. 3 for instance, that the maximum compression on the core was only 4½ tons per square inch when the gun was at rest, and the maximum tension only 5 tons, with an internal powder-pressure of 30 tons, and with the full action of the coil laid on with the requisite tension.

CHAMBERING.

A prominent feature in gun-construction at the present time is the extent given to what is called "chambering."

Although at first sight it does not appear to have any special reference to wire-guns, these have a very special reference to it, and to this extent, that if the principles of Wire-Gun Construction be properly carried out, chambering will be abandoned. Details on this subject will be developed in the treatise which the Author has in preparation for the press. He will here only give the conclusion which he has arrived at, with an example of the bearing Wire-Gun Construction has on chambering.

The conclusion is that chambering is simply a device for reducing the initial pressure to such an extent, that it will not overcome the inherent weakness of the guns of the present day. If then an unchambered gun can be constructed, which will not be more strained by an internal pressure of 30 tons than a chambered gun with a pressure of 20 tons, it is evident that the question of weakness disappears. If at the same time it can be proved that the unchambered gun will give a higher ballistic effect with a less charge of powder than the chambered gun, and that the gun is a much cheaper gun, a strong case will have been made out against chambering and in favour of unchambered wire guns.

The guns now about to be compared are represented by Figs. 21 and 22. The first, Fig. 21, is Sir William Armstrong's 13-inch wire gun previously mentioned. The second, Fig. 22, is

FIG. 21.

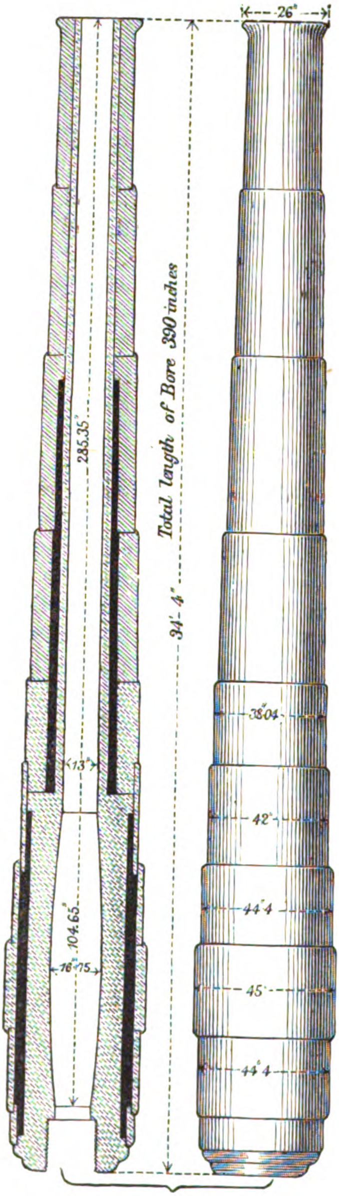
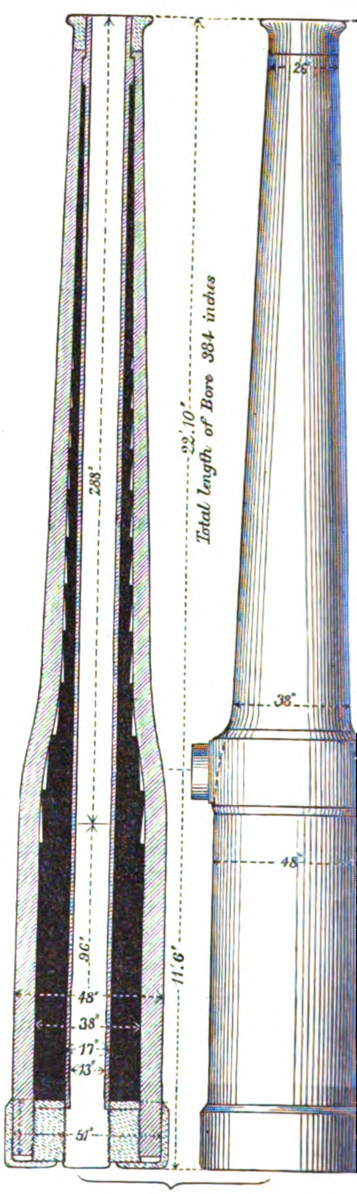

26"
Total length of Bore 390 inches
34' 4"
285.35"
13"
104.65"
16.75
38.04
42"
44.4
45
44.4

FIG. 22.


25"
22' 10"
Total length of Bore 384 inches
288"
38"
96"
48"
48"
38"
17"
13"
51"
11' 6"

a 13-inch wire gun designed by the Author. The first gun is chambered out for a length of several feet to a diameter of 16·75 inches; the second is not chambered.

In Sir William Armstrong's gun the wire is supposed to be laid on with a tension of 12·70 tons, which, as is shown in the Appendix, is the most advantageous tension for the gun, since it strains both the core and the coil to the same extent, namely, 20 tons per square inch with the internal pressure of 21 tons. In the unchambered gun, the wire is laid on with tension varying from 7·60 tons on the first wire to 8·90 tons on the last. The tension of the coil under fire, with an internal pressure of 33·5 tons per square inch is 20 tons uniform throughout, and the maximum tension in the core is 7 tons under fire, and the maximum compression 11·34 tons at rest.

	Tons.
In the first gun the coil supports	94·73
And the core	81·14
In the unchambered gun the coil supports	215·40
And the core	2·35

The ballistic power of these two guns will now be examined, making use of the Tables in Noble and Abel's "Researches on Explosives. No. II.—Fired Gunpowder."[1]

Sir William Armstrong's Gun.

The capacity of the chamber being 20,670 cubic inches, it will hold 752 lbs. of powder, gravimetric density = 1·00, but the charge being only 500 lbs. the actual gravimetric density is $\frac{500}{752} = 0 \cdot 6652$, which by Table X. in "Researches on Explosives" corresponds to an expansion of 1·504.

The capacity of the chamber is equal to 155·76 inches of the bore, and the length of the chamber being 104·5 inches, the number of expansions will be $1 + \frac{390 - 104 \cdot 5}{155 \cdot 76} = 2 \cdot 832$.

Now by Table XI. the total work which the powder is capable of performing is—

	Foot-tons per lb.
For 2·832 expansions	66·700
From which deduct 1·504 expansion	31·986
Total work for 1 lb. of powder	34·714

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[1] Philosophical Transactions, 1880, p. 203.

therefore for 500 lbs. there are 17,357 foot-tons as the total work of the gun.

UNCHAMBERED GUN.

Volume of the charge at gravimetric density 0·90 (= 1·111. expansion) $= 413 \frac{27 \cdot 5}{0 \cdot 90} =$ 12,725 cubic inches, which is equal to 95·08 inches length of bore. Consequently the number of expansions $= 1 + \frac{384 - 95 \cdot 08}{95 \cdot 08} =$ 4·038 expansions, and by Table XI. the total work which the powder is capable of performing is—

	Foot-tons per lb.
For 4·038 expansions	82·667
Deduct 1·111 expansion	9·717
Total work of 1 lb. powder	72·890

Therefore for 413 lbs. there are obtained 30,103 foot-tons as against 17,357 foot-tons from 500 lbs. of powder in the chambered gun.

The method of calculation here adopted is that employed by Messrs. Noble and Abel in their "Researches on Explosives," and is strictly comparative as regards the two guns. True, a still larger charge of powder might be put into the chambered gun, so as to raise the gravimetric density, or, with the same charge, part of the chambered space might be filled up with some inert material, but the result of this would be to increase the initial pressure, and consequently to strain the gun beyond the 20 tons per square inch fixed as the maximum. But if it be allowable to increase the strain beyond 20 tons per square inch on the one gun, it is so on the other. The total length of the guns is the same, the weight nearly the same, the cost of construction in time probably as 6 to 1, and in money as 4 or 5 to 1 in favour of the unchambered gun. The Author is prepared to prove the facts as to the relative strains on the guns; for the ballistic effect he has simply adopted Noble and Abel's method and data.

It may perhaps be asserted that this method does not apply to "slow burning powder;" that such powder has some way of acting whereby, whilst it greatly diminishes the initial pressure, it continues to generate gases in such a way as to keep up a high pressure as long as the shot is in the gun. This evidently seems to be considered the thing to be aimed at, for Colonel Maitland, in his Paper at the Society of Arts, speaks of "the great desideratum, a low maximum pressure long sustained."[1] No doubt it is, if the guns are weak, but just as reasonably might a locomotive engineer employ low-pressure steam without a cut-off, because he made the working parts too weak. What would be the result as regards economy of working? And so it is with a gun, which is as much a thermodynamic machine as is a locomotive.

Let it for a moment be granted that the desideratum is perfectly attained, that a powder is invented which will burn so as to give a perfectly uniform pressure throughout, which would be the perfection of a "low maximum, long sustained." Let the gun be five expansions long, then, if fired with a gravimetric density = 1, each lb. of powder would produce 91·385 foot-tons of work.

On the other hand, this would require a uniform pressure of 18·27 tons through the five expansions, so that when the shot left the muzzle, the five expansions would be filled with gas at 18·27 tons pressure. This gas would have a density of about 0·66, consequently the weight of powder would be 5 volumes × 0·66 = 3·30 lbs. as compared with the 1 lb. fired at a density 1.

There is really no mystery about slow-burning powder; it cannot do more work than quick-burning powder, and as regards ballistic effect it must weight for weight do less.

Much has been said and written about the effect of "dissociation," as well as about the heat stored up in an assumed liquid residue, which continues to supply heat to the gases after the explosion has ceased. The best answer to such assertions is the thermodynamic law, which may be thus expressed: "Any thermal machine, which works between given limits of temperature, gives the maximum useful effect when all the heat is received at the highest temperature and rejected at the lowest." This, applied to the gun, proves that the maximum useful effect is where all the heat is evolved before the shot moves, and at once disposes of the ballistic utility of slow-burning powder.

Let it not be said that results are now obtained far beyond those of a few years back. True, they are, but how? By increasing the weight of powder from one-sixth to one-third, and even one-half, the weight of the projectile.

Two very important inferences may be drawn from the investigations on which this Paper is based. The first is the paramount importance of correctly determining the tension for laying-on the wire. The second is the great advantage of using a core with a low modulus of elasticity. In the Author's opinion cast iron is a very suitable material. Its assumed brittleness is no objection. In practice it has been successfully used by the Author, and he is convinced that, as a material for the core, it is far superior to the expensive steel now so generally adopted. As regards its suitability for carrying the rifling, that is purely a matter of bearing surface, and as the days of studded projectiles are now at an end, no difficulty need be apprehended on this score.

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[1] Journal of the Society of Arts, vol. xxx., 1881–82, p. 334.

In conclusion, the Author fully admits that whatever merit or utility there may be in this Paper, it is due to the strict application of mathematical reasoning.

He may be told again that he has made mathematics his master instead of his handmaiden. If so, it is a master he is proud to serve. To be a master of mathematics is far beyond his aspirations, and probably beyond the power of any human being, but as a humble servant he respects, and has implicit faith in, his great master. He has endeavoured, with the assistance of one of far superior power, to investigate a very important though somewhat complicated problem, and the solutions obtained are in his belief beyond dispute.

Once more, perhaps, he may be allowed to insist on the grave error of supposing that theory and practice are or can be divergent. Hypothesis and practice may and very often do disagree, but theory never, unless it be a false theory. In the words of an acute thinker, "Theory is, in fact, no more than a system of laws, abstracted from experience; consequently if any apparent contradiction should exist between them, this could only argue that the theory had been falsely or imperfectly abstracted; in which case the sensible inference would be, not a summons to forego theories, but a call for better or more enlarged theories." The great German philosopher, Kant, has an essay "On the common saying, that such or such a thing may be true in theory, but does not hold good in practice," with an extract from which, as translated by De Quincey, the Author will conclude. He says "It is far more tolerable that an unlearned person should represent theory as superfluous for the purposes of his imaginary practice, than that a shallow refiner, whilst conceding the value of theory for speculation and scholastic uses, should couple with this concession the doctrine, that in practice, the case is otherwise; and that, upon coming out of the schools into the world, a man will be made sensible of having pursued mere philosophic dreams. In short, that what sounds well in theory is not merely superfluous, but absolutely false for practice. Now the practical engineer who should express himself in these terms upon the science of mechanics, or the artillery officer who should say of the doctrine of projectiles, that the theory of it was conceived indeed with great subtlety, but was of little practical value, because in the actual exercise of the art it was found that the experimental results did not conform to the theory, would expose themselves to derision. For, supposing that in the first case should be superadded to the theory of mechanics, that of friction, and that in the second, to the theory of projectiles were superadded that of the resistance of the air,—which in effect amounts to this, that if, instead of rejecting theory, still more theory were added, in that case the results of the abstract doctrine and of the experimental practice would coincide in every respect." [1]

The Paper is accompanied by numerous diagrams, from which the woodcuts in the text have been engraved.

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[1] Kant in his Miscellaneous Essays and Logic of Political Economy. By Thomas de Quincey, vol. xii., p. 333.

APPENDIX.

The following Appendix contains the principal formula for the construction and calculation of the strength of wire-guns, and a few examples of their application.[1]

I.

Formula for the strains in a tube or core of homogeneous metal.

Let f_0 be the internal normal force;
f_1 ,, external ,,
ρ ,, inner radius;
R ,, outer ,,
y ,, any intermediate radius.

Then tension at $y = t_y = \dfrac{f_0\,\rho^2\,(R^2 + y^2) - f_1\,R^2\,(\rho^2 + y^2)}{(R^2 - \rho^2)\,y^2}$ . . . (1)

by making $y = \rho$ and R successively,

$$t_\rho = \frac{f_1\,(R^2 + \rho^2) - 2\,f_0\,R^2}{R^2 - \rho^2} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (2)$$

$$t_R = \frac{2\,f_0\,\rho^2 - f_1\,(R^2 + \rho^2)}{R^2 - \rho^2} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (3)$$

or writing A for $\dfrac{R^2 + \rho^2}{R^2 - \rho^2}$,

$$t_\rho = f_0\,A - f_1 - (A + 1) \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (4)$$

$$t_R = f_0\,(A - 1) - f_1\,(A) \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (5)$$

It is sometimes convenient to use only the ratios of the radius, in which case, making $m = \dfrac{R}{\rho}$,

$$t_\rho = f_0\,\frac{m^2 + 1}{m^2 - 1} - 2\,f_1\,\frac{m^2}{m^2 - 1} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (6)$$

$$t_R = f_0\,\frac{2}{m^2 - 1} - f_1\,\frac{m^2 + 1}{m^2 - 1} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (7)$$

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[1] The method of calculating the various constants used in the following formulas is not given. It will be found with the complete investigations of the formulas, in a treatise on the subject which the Author is preparing for the press, and which will shortly be published by Messrs. E. and F. N. Spon. Charing Cross.

II.

SHRINKAGE-EQUATION.

To find the proper shrinkage between two hoops or between the Core and Coil.

Let R_n = outer radius of nth hoop;
ρ_{n+1} = inner " n + 1th hoop;
τ_n and T_{n+1} the respective tensions of the surface in contact;
k and k_1 the respective moduli of elasticity;
f_n the normal pressure between the hoops;

then adopting Wertheim's coefficient, and their being no longitudinal strain,

$$\tau_n = \frac{\rho_{n+1} - R_n}{R_n} k + T_{n+1} \frac{\rho_{n+1}}{R_n} \cdot \frac{k}{k_1} + \frac{f_n}{3} \left\{ \frac{\rho_{n+1}}{R_n} \cdot \frac{k}{k_1} - 1 \right\}.$$

And since R_n is very nearly $= \rho_{n+1}$,

and $\frac{\rho_{n+1} - R_n}{R_n}$ is the shrinkage per unit of length, which call S_n,

$$S_n\, k = T_{n+1} \frac{k}{k_1} + \frac{f_n}{3} \left\{ \frac{k}{k_1} - 1 \right\} - \tau_n \quad \ldots\ldots\ldots \quad (8)$$

whence $k = k_1$

$$S_n = \frac{T_{n-1} - \tau_n}{k} \quad \ldots\ldots\ldots\ldots\ldots \quad (9)$$

III.

Equation for finding the tension for laying on the wire, so that when the gun is under fire, with a given powder-pressure P, the whole of the coil may be uniformly strained to T.

The following is the equation:—

$$\text{Tension for laying at } y = \frac{T R \left\{ y - \frac{B \rho^2}{y} + C \right\}}{y^2 + B \rho^2} \quad \ldots\ldots \quad (10)$$

where B and C are constants for each particular gun depending on the ratios of the radius of the core and coil, and their respective moduli of elasticity.

IV.

Formula for determining the condition of the coil when completed and at rest.

When the gun is under fire and subject to the powder-pressure P, the whole of the coil is strained to T; but when there is no internal pressure, and the gun is at rest, the following is the formula:—

$$\text{Tension at } y = T - A' \left\{ 1 - \frac{B'}{y^2} \right\} \quad \ldots\ldots\ldots \quad (11)$$

where A′ and B′ are again constants depending on the ratios and moduli of elasticity.

V.

State of the coil under fire and at rest.

The first thing is to determine the normal pressure between core and coil, and then by (4) and (5) to find the states of the inner and outer surfaces, or by (1) the state at any intermediate point.

For the gun under fire—

$$f_0 = \text{P} \quad f_1 = \text{T}\ (n - 1),$$

where n is the ratio of inner and outer radius of the coil.

For the gun at rest—

$$f_0 = \text{T}\ (n - 1) - \frac{\text{P}}{\text{A}'' + \theta\ \text{B}''} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where A″ and B″ are constants as before, and θ is the ratio of the modulus of elasticity of the core to that of the coil.

VI.

To determine the condition of core and coil under fire, and when at rest, if the wire be laid on with uniform tension = θ.

The formula, when the gun is under fire, is

$$\text{Tension at } y = t_y = \theta\ \{1 + \left\{\frac{\alpha\ \rho^2 - \beta\ y^2}{2\ \beta\ y^2} \log \phi\ \left(\frac{\text{R}^2}{y^2}\right)\right\} + \gamma\ \frac{\text{R}^2 + y^2}{y^2} \times \text{P} \quad (13)$$

where α β and γ are constants depending on the ratios of the radii · $\phi\left(\frac{\text{R}^2}{y^2}\right)$ is a function of R and y, and P the internal powder-pressure.

This formula consists of two parts, the first of which gives the tension, so far as it is due to the tension of laying on, whilst the second shows the effect of internal pressure; consequently by making P = 0, the condition of the coil at rest is obtained.

For the condition of the core, find first the normal pressure between core and coil, which is given by the equation

$$f_\rho = \text{P}\ \{\text{A}'''\} + \theta\ \{\text{B}'''\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where A‴ and B‴ are again constants depending on the ratios of the radii and moduli of elasticity.

This expression is also in two parts, and gives the normal pressure under P, and making P = 0, it gives the normal pressure when at rest.

Having obtained these, the condition of the gun is found as before by (1), (4) and (5).

VII.

To find the "particular value" of θ which will give the same strain at the inner and outer surfaces of the coil.

Having first found the value of t_ρ and t_{R} in terms of θ and P by (13), and equating their value, θ is found in terms of P.

VIII.

To find the condition of the gun and the laying-on tension, on the hypothesis of the "extension" of the coil under fire being constant.

The equation for the tension of laying on is

$$t_y = \frac{A'''' + \left(\frac{R}{y}\right)^{\frac{2}{3}} \{B'''' + C'''' y^2\}}{D'''' + E'''' y^2} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (15)$$

the constants as before depending on the ratios of the radii, the moduli of elasticity, and the internal pressure, and also on the maximum tension of the outer wire of the coil when under fire.

In this equation, since t_y must always be positive, there is a minimum value of θ which it is first necessary to determine, and which is found by the condition

$$\theta > \text{or} = \frac{P}{N} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (16)$$

N being a constant depending on the ratios of the radii and the moduli of elasticity.

IX.

The condition of the coil when under fire is:—

$$t_y = \alpha\,\theta \left\{1 - \beta \left(\frac{R}{y}\right)^{\frac{2}{3}}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (17)$$

and at rest—

$$t_y = \alpha\,\theta \left\{1 - \beta\left(\frac{R^2}{y}\right)^{\frac{2}{3}}\right\} - \frac{\rho^2\,(R^2 + y^2)}{y^2\,(R^2 - y^2)} \times \frac{P}{B} \quad . \; . \; . \; . \quad (18)$$

the constants depending on the same conditions as before.

The conditions of the core are found as before, by first obtaining the normal pressures between core and coil, and then by the formulas (1), (4) and (5).

X.

To determine the ratios of the radii when the internal pressures, the maximum tensions of core and coil under fire, the maximum compression of core at rest, and the moduli of elasticity are given.

Assuming $y = m^2 = \frac{\rho^2}{\rho_1^{\,2}}$;

T = uniform strain of coil under fire, with an interior pressure P;

θ = ratio of the moduli of elasticity $= \frac{k}{c}$;

t' = maximum compression of core at rest;

t = „ tension „ under fire;

P = powder pressure;

a biquadratic equation is obtained:

$$y + a\,y^3 + b\,y^2 + c\,y + d = o \quad . \; . \; . \; . \; . \; . \; . \quad (19)$$

where

$$a = \frac{2\,T\,\{A + B\,\theta\} - C + \{D + E\,\theta\}\,t'}{2\,T\,\{F + G\,\theta\} + \{H + K\,\theta\}\,t'};$$

$$b = \frac{2\,T\,\{2\,A' + B'\,\theta\} - C' + \{D' + E'\,\theta\}\,t'}{2\,T\,\{F + G\,\theta\} + \{H + K\,\theta\}\,t'};$$

$$c = \frac{2\,T\,\{2\,A'' + B''\,\theta\} - C'' + \{(D'' + E'')\,\theta\}\,t'}{2\,T\,\{F + G\,\theta\} + \{H + K\,\theta\}\,t'};$$

$$d = \frac{2\,T\,\{A''' + B'''\,\theta\} - \{D''' + E'''\,\theta\}\,t'}{2\,T\,\{F + G\,\theta\} + H\,\{+ K\,\theta\}\,t'};$$

and the constants depend on the internal pressures and the assumed tensions.

Having found y or m^2, the ratio of the radii of the core is obtained, or $\frac{\rho_1}{\rho}$.

To find the ratios of the radii of the coil, or $n = \frac{R}{\rho}$, the condition must be:

$$n = \frac{P + 2\,T - t}{2\,T} + \frac{P + t}{2\,T\,m^2}.$$

It must be observed that a positive value of $y > 1$ is not always possible, as t' cannot be arbitrarily assumed.

XI.

A few examples will now be given of the application of these formulas.

XII.

To find the tension of laying on the wire, the effect of the normal forces being taken into account, in a gun of the following dimensions, and under the following conditions:—

ρ_1 = inner radius of core . . . . 6 inches;
ρ = outer " " . . . . 8 "
k = modulus of elasticity of core . . . . 4,500 tons;
e = " " wire . . . . 22,000 "
P = powder-pressure . . . . 30 " per square inch;
T = uniform strain on wire with P . . . . 20 " "
t = maximum tension of core under fire . 5 " "

We have $m = \frac{\rho}{\rho_1} = \frac{8}{6} = 1 \cdot 3333 \therefore m^2 = 1 \cdot 7777.$

To find R—

$$\text{By (6)} \quad t_\rho = f_0\,\frac{m^2 + 1}{m^2 - 1} - f_0\,\frac{2\,m^2}{m^2 - 1};$$

but $t_\rho = 5$ by assumption $f_0 = 30$, and $f_0 = T\,(n - 1)$

$$\therefore 5 = 30 \cdot \frac{2 \cdot 777}{0 \cdot 777} - 20\,(n - 1)\,\frac{3 \cdot 555}{0 \cdot 777},$$

whence $n = 2 \cdot 1137$, and

$$R = n\,\rho = 16 \cdot 91, \text{ the outer radius of the coil.}$$

The equation for tension of laying on at y is—

$$(10) \quad \text{or} \quad t_y = \frac{\mathrm{T\,R}\,\{y - \mathrm{B}\,\rho^2 + \mathrm{C}\}}{y^2 + \mathrm{B}\,\rho^2};$$

and the constants are—

$$\begin{aligned} \mathrm{T\,R} &= 338\cdot2 \\ \mathrm{B} &= -\ 0\cdot8835 \\ \mathrm{C} &= -\ 15\cdot004 \end{aligned}$$

Therefore (10) becomes—

$$t_y = \frac{338\cdot2\left\{y + \dfrac{56\cdot542}{y} - 15\cdot004\right\}}{y^2 - 56\cdot542}.$$

From which the following values are obtained:—

Value of y.	8·0	8·1	8·2	8·4	9·0	10·0	12·0	16·91
Laying-on tension .	2·875	2·739	2·776	2·999	3·815	4·835	6·634	7·770
Tension of coil under fire . .	20·00	20·00	20·00	20·00	20·00	20·00	20·00	20·00
Tension of coil at rest	−13·52	−12·85	−12·22	−10·89	−7·77	−3·66	+1·70	+7·770

The last line is obtained from (11), which becomes, by inserting the proper constants:—

$$\text{Tension at } y = 20 - 6\cdot136\left\{1 + \frac{285\cdot93}{y^2}\right\}.$$

For the core under fire—

Tension of inner surface by assumption $t_\rho = 5\cdot0$;

„ outer „ by (7)—

$$t_{\mathrm{R}} = f_0\,\frac{2}{m^2 - 1} - f_1\,\frac{m^2 + 1}{m^2 - 1} = 30 \times \frac{2}{0\cdot7777} - 22\cdot274\,\frac{2\cdot777}{0\cdot777} = -\,2\cdot235.$$

And for the core at rest—

$$f_0 = 0 \quad f_1 = \mathrm{T}\,(n - 1) - \frac{\mathrm{P}}{\mathrm{A}'' + \theta\,\mathrm{B}''} = 22\cdot274 - 21\cdot262 = 1\cdot010.$$

Therefore:—

$$\text{Tension of inner surface} = -\,1\cdot010 \times \frac{3\cdot555}{0\cdot7771} = -\,4\cdot600;$$

$$\text{„ outer „} = -\,1\cdot010 \times \frac{2\cdot777}{0\cdot777} = -\,3\cdot590;$$

From the above values Fig. 3, p. 92, is constructed.

Fig. 4 shows the corresponding values when the modulus of the core is the same as that of the coil, and are obtained from the same equations, making $k = c$.

Fig. 1 is constructed from the original formula, which did not take into account the effect of the normal force on the extensions.

Fig. 1 is for the case when k = 4,500, c = 22,000, and No. 2 when $k = c$.

Comparing Fig. 1 with Fig. 3, and Fig. 2 with Fig. 4, it is seen that wh
normal forces are taken into account, a great alteration takes place in th
sions, whilst in the case of equal moduli, Figs. 2 and 4, they have no effect

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XIII.

To find the condition of the gun when the wire is laid on with a u
tension.

The equation No. (13), by the insertion of the proper value of the con
for the same gun, and under the same conditions as above, becomes

$$\text{Tension at } y = \theta \left\{1 - \frac{28\cdot264}{y^2} + \frac{1}{2}\right\} \log \frac{61\cdot506}{0\cdot2681\, y^2 - 15\cdot11}\right\} + 6\cdot135 \left\{\frac{285\cdot}{y^2}\right.$$

the first part representing the tension due to θ, and the second the tensi
to the powder-pressure.

From this the following values of tension at rest and under fire hav
calculated.

Value of *y*.	Uniform Tension of Laying on per Square Inch.								
	2 Tons.		4 Tons.*		6 Tons.*		10 Tons.*		20 T
	At Rest.	Under Fire.	At Rest.	Under Fire.	At Rest.	Under Fire.	At Rest.	Under Fire.	At Rest.
8·0	−4·45	29·121	−8·90	24·67	−12·36	20·22	−22·26	14·31	−44·52
8·1	−4·02	28·85	−8·03	24·74	−12·04	20·73	−20·07	12·80	−40·14
8·2	−3·64	28·58	−7·28	24·94	−10·93	21·29	−18·21	14·01	−36·42
8·6	−2·55	27·30	−5·10	24·76	− 7·64	20·21	−12·74	17·11	−25·18
9·0	−1·50	26·00	−3·60	24·02	− 5·40	22·40	− 9·00	18·80	−18·00
10·0	−0·60	23·07	−1·21	22·47	− 1·81	21·86	− 3·02	20·65	− 6·04
13·0	+1·05	17·56	+2·10	18·62	+ 3·15	19·66	+ 6·24	22·75	+11·48
16·91	+2·00	14·27	+4·00	16·27	+ 6·00	18·27	+10·00	22·77	+20·00

The negative signs denote compression.

For the condition of the core the normal pressure must be found
which, inserting the proper values of A‴ and B‴, becomes:—

$$\text{Normal pressure} = 0\cdot7098\ P + 0\cdot19873\ \theta;$$

and making P = 0, gives the normal pressure at rest.

From this the following Table is constructed:—

	Laying-on tension .	0	4 *	6 *	6·618	10 *	13	15	20 *
Under fire	Normal force	21·294	21·988	22·486	22·606	23·281	23·877	24·274	25·268
	Tension of inner surface	9·79	6·62	4·39	3·79	0·96	− 2·01	− 3·83	− 8·37
	Tension of outer surface	1·08	−1·40	−3·17	−4·00	−5·86	− 8·13	− 9·36	−13·10
At rest	Normal force	0	0·794	1·192	1·315	1·987	2·583	2·980	3·974
	Tension of inner surface	0	−3·63	−5·45	−6·01	−9·08	−11·81	−13·63	−18·17
	Tension of outer surface	0	−2·84	−4·26	−4·69	−7·10	− 8·13	−10·65	−14·20

From the columns marked * in these Tables, Figs. 5, 6, 7 and 8 have been constructed.

In order to find the "particular tension" of this gun, that is to say, the laying-on tension which will give equal strains under fire to the first and last wire of the coil, these tensions must be found in terms of θ, and equated.

Thus—

Tension of first wire $- 2{\cdot}214\ \theta + 33{\cdot}54$,
„ last „ $\theta + 12{\cdot}269$;

whence $\theta = 6{\cdot}618$, the laying-on tension required, and by making use of this in the formulas, the following Table of tensions at rest and under fire is obtained:—

y.	8·0	8·1	8·2	8·6	9·0	10·0	13·0	16·91
Tension at rest .	−14·73	−13·45	−12·07	−8·38	−5·94	−0·95	+3·47	6·618
Tension under fire	18·86	19·45	20·15	21·52	21·84	22·74	20·00	18·86

Thus although the extreme wires are strained to the same degree, yet the tension of the coil is not uniform throughout. It rises to 22·74 tons at 10 inches, and then falls again to 18·86 at 16·91, the outer radius.

Fig. 23, p. 118, represents the condition of the gun when this "particular tension" is used.

XIV.

Wire laid on so that the "extension" under fire shall be constant.

The formula (15) is given for this case, and the constants for this gun are—

$$A = -3330 \quad B = 1{,}291 \quad C = 11{,}423 \quad D = -27{\cdot}95 \quad E = 0{\cdot}4943.$$

The tension of the outside wire under fire is 23·09 tons per square inch, which is the minimum that can be adopted, and it gives the laying-on tension of the first wire = 0. Any lower value would give a negative result for the first wire.

FIG. 23.

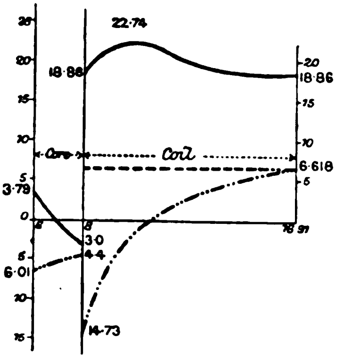


Fig 20, p. 101, represents the gun under this condition.

This formula is not practically useful, as it is based upon a false assumption, as has been already explained.

XV.

Formula for determining the ratios m and n, when the maximum tensions are given.

Assuming the following data—

$P = 21$ tons; $T = 17 \cdot 2$ tons; $t = 5$ tons; $t' = -7 \cdot 5$;
$k = 7{,}000$; $c = 21{,}000$.

The following are the constants—

$$\gamma = 1 \cdot 4651 \qquad \beta = 0 \cdot 7558$$

$$\therefore n = 1 \cdot 4651 + \frac{0 \cdot 7558}{m^2}.$$

To find m—

$$y^4 - 0 \cdot 45086 \quad y^3 - 4 \cdot 2094 \quad y^2 + 2 \cdot 629 \quad y + 1 \cdot 0272 - 0;$$

from which $y = 1 \cdot 802$

$$\therefore m = \sqrt{y} = 1 \cdot 3424;$$

and $$n = 1 \cdot 4651 + \frac{0 \cdot 7558}{1 \cdot 802} = 1 \cdot 8845.$$

Therefore the radii are—

$\rho_1 =$ inner radius of core $= 8 \cdot 375$ inches.
$\rho =$ outer radius $= m\,\rho_1 = 11 \cdot 242$ „
$R =$ „ of wire $= n\,\rho = 21 \cdot 186$ „

If, other things remaining the same, take $t' = -2$

then $m = 1 \cdot 054$; $n = 2 \cdot 146$;
$\rho_1 = 8 \cdot 375$;
$\rho = 8 \cdot 824$;
$R = 18 \cdot 932$.

In both the above cases $\frac{k}{c} = \frac{1}{3}$. If, now, $k = c$, it will be found that there is no value of y greater than 1, which satisfies the equation, showing that when the moduli of elasticity are equal, it is impossible to make a gun to satisfy the above conditions.

A gun might, of course, be made of the above dimensions, and with the moduli equal; but if it be submitted to calculation, it will be found that the compression of the core at rest would be $47 \cdot 89$ tons, instead of $7 \cdot 5$, as required.

A brief analysis of two wire guns, the dimensions of which are stated in the Report of the United States Ordnance Board, will now be given. Senate Document 178, Part 2 of 1882.

Captain Schultz's 34-Centimetre Gun.

The first is Captain Schultz's 34-centimetre (13·39-inch) gun, made with a steel tube 4·32 inches thick at the powder chamber, on which is coiled a layer of 4·53 inches of steel wire 0·118-inch in diameter, with a uniform tension of 45 tons per square inch.

The following are the radii at the powder chamber:—

$\rho_1 = 7{\cdot}285$;
$\rho = 12{\cdot}195$; $k = c$;
$R = 16{\cdot}725$;
P, the powder-pressure, is given at 18 tons per square inch.

Fig. 24 shows the condition of this gun under fire and at rest.

It will be seen, that if the elastic limit of the wire extended so far, it would be strained under fire to:—

53·43 tons per square inch;
and at rest to 45·00 „ „ the laying-on tension.

The core at rest, would be compressed to

38·75 tons per square inch at the inner surface;
and to 26·29 „ „ „ outer „

and under fire it would never be out of compression.

As, however, the elastic limit of the wire was probably not more than 23 tons per square inch, this would be practically the laying-on tension, as all above that would only go to stretch the wire permanently.

Even under these circumstances, the tension under fire would be (Fig. 18, p. 99):—

At the inner surface . . . . 28·14 tons;
„ outer „ . . . . 36·23 „

or 13 tons above the elastic limit, and a new condition of the gun would arise at each fire, until the tension of the outer wire did not (under fire) exceed the elastic limits; but in this state it is impossible to say what would be the condition of the other parts of the gun. It might be safe, or it might not. At any rate, it is very undesirable that a gun should be thus constructed and left to adjust itself by the process of firing.

The laying-on tension, which would give the tension of the outer wire under fire 22 tons per square inch, would be 13·57 tons, and the condition of the core with this tension is shown by Fig. 25, but it will be seen that the tube or core is doing too little work under fire. In fact, it is only taking up 40 tons out of 131, leaving 90 tons to the coil.

The "particular tension" for this gun is 6·35 tons, and when thus constructed, the coil is nearly uniformly strained to 14·78 tons per square inch, the maximum strain on the core under fire being 20·97 tons.

In this state:—

	Tons.
The core supports	63·54
And the coil	67·59
Total internal pressure	131·13

The uniform laying-on tension which would strain the core and coil equall

FIG. 24.

$\frac{K}{C} = 1$

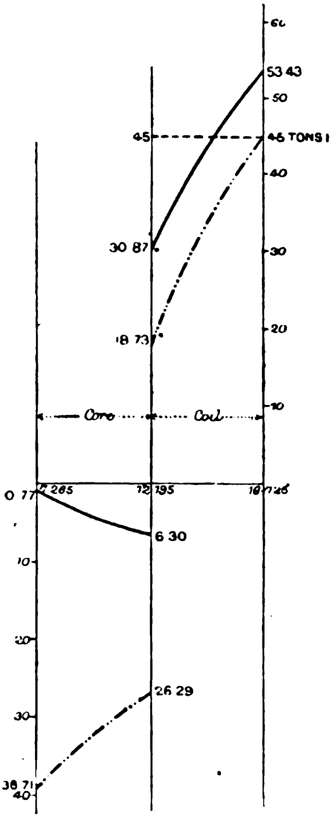
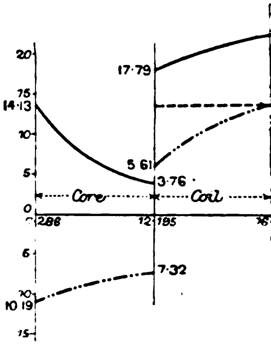


FIG. 25.



20 tons per square inch, with an internal pressure of 19·87 tons, may be fo to be 10·7 tons per square inch. Fig. 19, p. 101, represents the gun under condition.

This example shows very clearly the great importance of determining proper tension of laying on the wire.

SIR W. ARMSTRONG'S 13-INCH GUN.

The second example is the 13-inch gun of Sir William Armstrong, as described in the same document.

It is a 13-inch steel gun, chambered out to 16·75 inches. The powder-pressure is not stated, but judging from the pressures given for the 10·236-inch gun, it may be taken at 21 tons per square inch. Nothing is said about the laying-on tension, but probably it was a uniform tension.

The condition of the gun has therefore been calculated for the tensions of 10, 20, 13·4, and 6·18 tons.

The following are the radii:—

$$\rho_1 = 8{\cdot}375;$$
$$\rho = 16{\cdot}650; \quad k = c;$$
$$R = 21{\cdot}750.$$

Fig. 26 shows that if the laying-on tension is 10 tons per square inch, the coil under fire is strained to 15·74 and 17·29 tons at the inner and outer surfaces, and the core to 21·38 at the inner, and 5·94 at the outer surface.

FIG. 26.

$\frac{C}{K}=1$

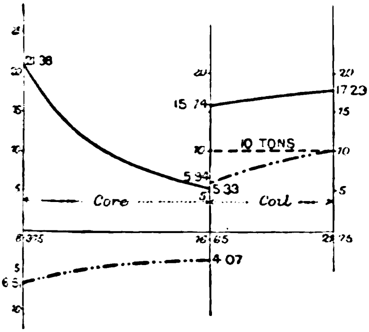


FIG. 27.

$\frac{K}{C}=1$

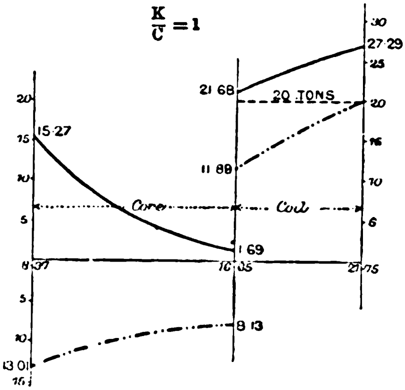


Fig. 27 shows that with a laying-on tension of 20 tons, the coil is strained to 27·29 and 21·68 tons, and the core to 15·27 and 1·67 ton.

Fig. 28 shows the condition with the wire laid on with the "particular tension" of this gun, which is 6·18 tons per square inch. It shows that the strain on the coil under fire is very nearly uniform, and is the same at the inner and outer radii, namely, 13·48 tons per square inch, and does not vary anywhere more than 0·41 from this.

The core is, however, strained to 24·25 tons at the inner surface, which is too great for the elastic limit of the material.

The laying-on tension has therefore been calculated, which would give a maximum strain on the core under fire of 20 tons per square inch, and which is found by formula (4). Making

$$f_0 = P, \text{ and } f_1 = 0{\cdot}2443\,\theta + 12\,P,$$

maximum strain of core $= P\,A - \{0{\cdot}2443\,\theta + 12\,P\}\,A + 1$

$$= 1{\cdot}346\,P - 0{\cdot}6515\,\theta,$$

also max. strain of coil $= 0{\cdot}3472\,P + \theta$;

and if these strains are to be equal and each 20 tons—

$$\theta = 0\cdot6048\ P, \text{ and since } P = 21,$$
$$\theta = 12\cdot70.$$

Adopting this tension of laying on, Fig. 29 is obtained, where the core and coil are each strained to 20 tons under fire.

FIG. 28.

$\frac{K}{C} = 1$

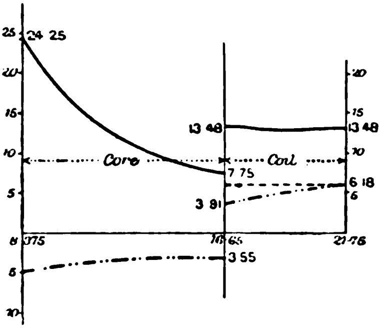
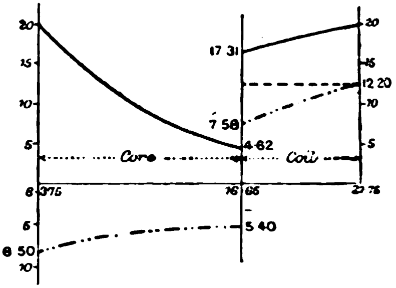


FIG. 29.



The relative distribution of the internal force between the core and coil in these various conditions of construction, is worthy of observation. The total force to be resisted is 21 × 8·375 = 175·82 tons, and it is borne in the following proportions:

——	Tension of Laying-on Wire.			
Proportion of force .	Tons. 10	Tons. 20	Tons. 6·18	Tons. 12·70
Core	93·32	52·25	109·32	81·14
Coil	82·55	123·57	66·55	94·73
Total . . .	175·87	175·87	175·87	175·87

Comparing the average strains per square inch on the core and coil, it will be found to be:—

——	Tension of Laying-on Wire.			
Average strains per square inch . .	Tons. 10	Tons. 20	Tons. 6·18	Tons. 12·70
Core	11·28	6·33	13·21	9·28
Coil	16·19	24·24	13·05	18·58

Thus, with the "particular tension," they are equally strained: with a tension of 10 tons the coil is strained 50 per cent. above the core; with a tension of 12·70 tons, 100 per cent.; and with a tension of 20 tons, 300 per cent. above the core.

In every case there is the serious defect, that owing to the thickness of the core, it is impossible to make it do its fair share of work. Although with the "particular tension" it is doing so, yet this is a tension which cannot be applied, because it gives too high a tension to the inner surface of the core under fire.

Probably Fig. 29 represents the best possible form of construction of this gun; and yet in this case the coil is strained to twice the extent per square inch of the core.

Compare this gun in its best condition, with a gun constructed with a yielding core, and the wire laid on with a varying tension, so as to be uniformly strained to 20 tons per square inch under fire.

Let the modulus of elasticity of the core be 7,333 tons, that of the coil 21,000 tons, and let the internal pressure be the same as in the Armstrong gun, 21 tons per square inch.

The dimensions of the gun would be as follow:—

$$\rho_1 = 8\cdot375;$$
$$\rho = 11\cdot225;$$
$$R = 19\cdot463.$$

FIG. 30.

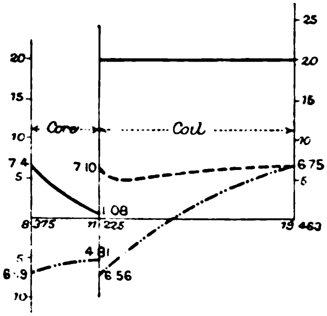


Fig. 30 represents the condition of this gun.

	Tons.
The uniform strain of the coil under fire is	20·0
The maximum strain of the core under fire is	7·4
The maximum compression at rest	6·19

The laying-on tension varies from 7·10 to 6·75 tons.

Now this gun is no more strained by the internal pressure than the Armstrong gun, although it is 2·30 inches thinner, and it would be a very much cheaper gun, inasmuch as it is made of cast iron instead of steel.

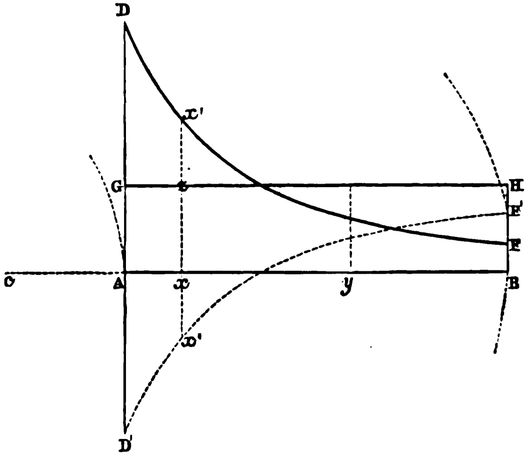
As regards safety the guns would be equal; as regards economy, both of time and money, the proposed gun would probably cost one-fifth of the cost of the other, and be made in one-fifth of the time.

Discussion.

Sir J. W. Bazalgette. Sir J. W. **Bazalgette**, C.B., President, said the subject to which the attention of the members had been drawn was one of peculiar interest at the present time, and the Author in bringing it before them had evidently bestowed upon it much thought and labour, and he was quite sure they would agree in voting him thanks for the very able manner in which he had introduced his views and supported them by mathematical calculations.

Mr. Longridge. Mr. **Longridge** said the principle upon which his investigations rested was very analogous to the second law of motion; it was, in fact, this: that when two or more strains acted upon a body at the same point, the aggregate effect was simply the algebraical sum of the separate effects of the separate strains. To apply this to the problem of finding the laying-on strain at any point, so that when under a given internal force the coil should be uniformly strained throughout. Putting aside the question of the core, in Fig. 31—

Fig. 31.



Let A B be the thickness of the coil;

ρ the inner radius C A;

R the outer radius C B;

then, under the action of an internal force f_0 and external force zero, the tension of the wire at x, if put on without internal tension, would be represented by the ordinate $x x'$ of the well-

Mr. Longridge. known curve D E. But it was required that the tension at x and at every other part under fire should be $x\,z$, a constant quantity. It was evident, therefore, that from the tension at every ordinate must be subtracted a quantity $z\,x'$ equal to the difference between $x\,x'$ and A G, or the part at x must be in an initial state of compression $x\,x'' = x\,x' - x\,z$ when the gun was at rest. If therefore the curve D′ E′ was drawn similar to D E, with G H as the axis of abscissas, evidently this was the initial state of the strained coil which, when f_0 was applied internally, would give the uniform strains of the line G H. But the problem was to find the tension with which any wire, at y for instance, should be put on, in order that when all the rest of the wire was put on the tension at x should be exactly $x\,x''$. By the formula the normal force in the finished coil could be found which was exerted at y by all the coils between y and B upon the remaining coils between y and A, which call ϕ. Now it was clear that the condition at x was not altered if all the coils were removed between y and B, and were replaced by the normal force ϕ. Consequently an expression could be obtained for the tension at x in which the effect of the force ϕ might appear by itself, and if this was called a function of the other variables ρ' R′ &c., it would be of the form

$$\phi_{,}\, f_{,}\, (\rho'\ \mathrm{R}'\ \&c.),$$

and the whole tension at x would be of the form

$$t_x = f_{,}\, (\rho_{,}\ \mathrm{R}_{,}\ \mathrm{C}\,x\ \mathrm{C}\,y\ \&c.) + \phi\, f_{,,}\, (\rho'\ \mathrm{R}'\ \&c.),$$

the latter part representing exclusively the effect of the normal force ϕ. If then in the expression thus obtained ϕ was made $= 0$, the result would be the state of strain at x in the remainder of the coil when at rest, and the coils external to y removed. In this expression, if x be made $= y$, the state of strain of the outside or last wire laid on at the distance A y from A was obtained, and this was of course the laying-on tension at y which was required to be found. That was the principle on which the formula had been constructed. Of course it was impossible to do more than simply explain the principle; but he thought it could be understood. There were one or two remarks which he should like to make. One was with respect to the great importance of having a thin core. He had prepared Fig. 32, in which the core was double the thickness of all the others—4 inches, instead of 2 inches; in that there was for the first wire a laying-on tension of 26 tons instead of 2·87 tons, and the compression of the coil at rest was 11 instead of 4·6 tons.

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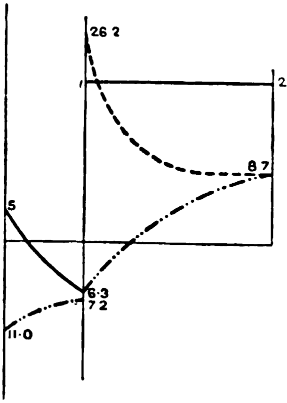
FIG. 32.



Mr. Longridge. Thus there was a great improvement in having the thin core; in fact, if the external coil was put on a core of no thickness there would be no pressure between them. That was, of course, impossible; but the same object might be accomplished, if desirable, by a construction which he need not now describe. He could make a gun with a thickness of core of either 2 inches or 4 inches, in which when the gun was complete there would be no tension upon the core, even when fired; the whole tension would be thrown upon the coil. There were one or two remarks he wished to make upon Captain Schultz's gun. If the wire was put on with a uniform tension of 45 tons per square inch, and if its elastic limit extended so far. Fig. 24 would represent the state of the gun. Under fire it would be strained to 53·43 tons on the outside and 30·87 on the inside of the coil, whilst the steel core would absolutely feel no tensile strain. It would have 0·7 ton compression on the inside, 6·3 tons compression on the outside under fire; 38·7 tons compression on the inside, and 26·29 tons on the inside when at rest, consequently the core would be of no use, supposing it were put on as it was stated in the report above mentioned. On the other hand, assuming the elastic limit of the wire to be 22 tons, and that the tension under fire should never exceed that limit, it could be calculated what should be the laying-on tension; it would be 13·7 tons per square inch. Under those conditions the outside of the wire would be strained to 22 tons, and the inside to 17·79; and for the core, the inside strain would be 14·17 under fire, and 10·79 tons compression at rest (Fig. 25, Appendix). This showed the enormous difference it made whether the wire was put on with the proper calculated tension or whether it was put on haphazard. The other diagrams, Figs. 26 and 27, represented the gun of Sir William Armstrong. In the first case it was supposed that there was a uniform laying-on tension of 10 tons; Fig. 26 represented the strains which would then take place; Fig. 27 represented it with a laying-on tension of 20 tons; and Fig. 28 represented what he called the particular tension for that gun, that was the uniform tension which would strain equally the coils both of the inside and the outside. Fig. 29 represented what he thought was the

Mr. Longridge. best possible tension for this gun, inasmuch as it strained both the wire and coil to the same extent, 20 tons per square inch. The laying-on tension was 12·7 tons per square inch, and that was the gun which he had compared as of equal strength with that represented in Fig. 30. To demonstrate how a gun might be constructed of the same strength as that of Sir William Armstrong's, he had prepared diagram Fig. 30, showing a cast-iron core with 2 inches less thickness of wire upon it, that was to say, the total thickness of the gun was 2 inches less than Sir William Armstrong's. That gun would be strained 20 tons uniformly throughout, laying-on tension 7·1 and 6·75 tons, tension on core 7·4, and maximum compression at rest 6·1 tons per square inch; therefore that gun, 2 inches thinner than the other, would be one of equal strength. Further details respecting these guns had been given in the Appendix. With regard to the question of heating, he must cry *peccavi*. He had expressed strong opinions that the actual heating from the gases had very little to do with the heating of guns, and stated that, in his opinion, it was more due to the conversion of energy than to heat. He had investigated that at considerable length, and found where he made the mistake; he had calculated upon the heat which was imparted by contact, and left out the important effect of radiant heat. He found, taking this into account from the best data he could get, that something like 80 per cent. of the heat arose from the effect of radiant heat; the rest of the amount was due partly to the friction of the gases and the projectile, and partly, as he supposed, to the conversion of energy, but that conversion of energy was comparatively small. From heat it was probably 80 per cent.; friction of gases and shot about 12 or 15 per cent.; and from conversion of energy no more than 5 or 8 per cent.; so that he was wrong altogether in what he stated formerly. The details of these investigations would be found in his book, which would shortly be published. He had only one other remark to make, and that was with regard to what he had said about powder—that slow-burning powder was simply a device for weak guns. Colonel Brackenbury, Superintendent Royal Gunpowder Factory, in a Paper read recently at the Royal United Service Institution,[1] on "Gunpowder considered as the Spirit of Artillery," said: "We constantly hear that a gun has been produced which will do this or that, yet it is not the gun which does it, but the gunpowder. The gun is only a tube to concentrate the action of the powder and guide

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[1] Journal of the Royal United Service Institution, vol. xxviii., p. 379.

Mr. Longridge. the projectile. There is not a single gun actually adopted for service in any country which is not, by its weakness, a hindrance to the full action of the 'Spirit of Artillery.' When gun-makers say, as they frequently do, that their gun will produce a certain effect 'provided that a suitable powder be found for it,' they mean 'provided that the strength of the gunpowder be restrained, "cribbed, cabined and confined" to suit the weakness of the gun.' We sometimes see in human life a great and strong spirit tear to pieces a feeble frame which contains it, and we do not say 'What a pity that the spirit is so strong,' but rather 'How sad that the body is so weak.' In the case of artillery we are always subduing and taming the spirit instead of strengthening the body. This may be necessary under existing circumstances, but, if so, the circumstances are unfortunate, and stand in the way of getting the most value out of the Spirit of Artillery." He entirely concurred with Colonel Brackenbury's remarks, and his object had been to show how to make a gun with which to conquer gunpowder.

Colonel Maitland. Colonel MAITLAND, R.A., Superintendent of the Royal Gun Factories, said, as regarded the powder question he did not quite agree with his fellow-superintendent, Colonel Brackenbury. He found that powder-makers generally liked very strong guns because they did not make their powder very regular or keep it under control; but he thought by working together they had attained with their slow-burning powder a very great deal more energy per ton of gun, which was the simplest way of measuring it, than was ever done with the quick-burning powder. If that theory was right, why should they not at once take gun-cotton? That would be the next step; if they could once control that, it would be much better than powder, taking up much less space for an equal amount of gas. With regard to the construction of these guns he thought the Author had given up experimenting with them, when he gave the lecture at the Society of Arts, but the Author had since informed him that he had not done so. In that lecture Colonel Maitland mentioned that he understood the longitudinal strength had not been fully provided for by the Author, and he must confess that he did not see where the longitudinal strength came in. He should not like to stand behind that gun (Fig. 22). The trunnions appeared to have no means of attachment, though no doubt that could be provided for, and the breech would certainly come away, because there was scarcely anything to hold it. Captain Schultz's 34-centimetre gun gave way longitudinally the first time it was fired. It was made not exactly on that principle;

Colonel Maitland.

it had twelve large bolts, 6 or 7 inches in diameter, placed round the gun from trunnion to breech. The first time it was fired six sharp cracks were heard, and it was found that six bolts had gone, so that the longitudinal strength must be more looked after than had hitherto been done by those who advocated the very large employment of wire. As to the gun which burst on board the "Daring," many guns of exactly the same nature had been fired many hundred rounds, and that particular gun had been fired about one hundred and sixty rounds. The gas corroded the material which was wrought-iron welded together in a weak place, and there were unfortunately two very bad welds in the gun nearly over one another. When the gas got into the interior it acted, of course on a larger surface, and the breech of the gun was blown off. He did not think it could be fairly said that it was theoretically weak, because there were so many hundreds of guns, of similar construction, which had been fired thousands of rounds. He did not know that there was any other point he need enlarge upon. As soon as the Author's book came out he would thoroughly go into it, and would try what could be done with wire put on on his principle, and he was sure the country would be very much indebted to him if it turned out to be successful.

Mr. Longridge.

Mr. J. A. LONGRIDGDE thought some misapprehension might be avoided if he was allowed to point out on Fig. 22 his proposed method of getting longitudinal strength. The inner part was the inner tube or core, and the black was the wire coil. Then came the outer jacket or casing, which extended from the breech to the trunnions, and was continued forward to the muzzle of the gun. In the 13-inch gun it was proposed to make this outer jacket 6½ inches thick between the breech and trunnions, of cast-iron,—the jackets were made thick to give weight to the gun, and protection to the wire, and had nothing to do with the circumferential strength of the gun. It was to give the weight required on account of the recoil, and to afford protection to the wire. Between the trunnions and the breech it had also to furnish longitudinal strength. Supposing that the inner tube and wire coil was slipped in from the end, then the heavy steel ring, which carried the breech-plug, was screwed into the outer casing; it did not touch the inner tube nor the wire. A small space was left between them, so that the inner tube was perfectly free to expand without bringing any strain upon the outer casing. The ring was held to the casing by the internal and external screw. These had fully the same length as the breech-plug, and a greatly increased diameter. There were two of them, and the breech-plug

Mr. Longridge. was cut away for one-half of the threads, in order to draw the breech out; and it was evident that long before this gave way the breech-plug must be blown out. Experience had shown that cutting away one-half of the thread left sufficient strength to resist the full powder-pressure; therefore it was certain that if anything gave way, it must be the cast-iron between the breech and the trunnions. There the cast-iron was 6½ inches thick. Assuming a pressure of 30 tons per square inch on the inside of the gun, multiplied by the area, and dividing that by the annular area, the result would be a strain of 4 tons per square inch only upon the cast-iron. The cast-iron would never give way under that pressure. If it was thought too great a pressure, let the area be doubled and make it 2 tons. The thing was, to take all the longitudinal strain off the inner part of the gun, the function of which was to resist the bursting strain, and to put it on the outside, the function of which was to resist the longitudinal strain. That was the principle for which he had contended for the last thirty years. That was evidently an efficient and complete way; but there were other ways of doing it, some of which he had previously exhibited. He wished to say one word about the assertion that he made, that Captain Schultz had overcome the assumed difficulty, and to which Colonel Maitland replied that Captain Schultz's 34-centimetre gun had failed in the first round. In that gun, the connection between the trunnion and the breech was made by twelve bolts, not 7 inches and 8 inches in diameter, but 3¾ inches in diameter. Those bolts, when the gun was fired, naturally gave way. The aggregate area of those bolts was 132 inches; the aggregate area of the bore was 142 inches; consequently if a pressure of 20 tons were put on the inside, thus causing a sudden strain of 21½ tons per square inch upon those bolts, it was no wonder that they had given way. But there was another difficulty in that system, the same difficulty which Captain Blakeley found in the first gun that he made. It was impossible to get a uniform strain upon twelve bolts. The consequence was that one failed after the other, for the reason that they were strained far beyond their strength. He had been in correspondence with the late Captain Schultz, who told him that he perfectly agreed with him about the longitudinal strength, that he did not find the slightest difficulty about it, and in subsequent guns there had been no difficulty. He would add one word to what he had said with respect to the heating of the guns. When his book came out it would be seen that there was not the slightest cause for uneasiness with regard to the tension of the

Mr. Longridge. wires. The effect of the heating was so exceedingly minute, that it was not of the least consequence.

Mr. Barlow. Mr. W. H. Barlow, Past President, remarked that it might be expected of him to say a few words on this matter, because during the last three years he had been rather continuously engaged upon it. He did not know that he need advert to that part of the subject with which the Paper opened, namely the theory—that had been pretty well thrashed out before. The Author referred to the investigations of his father, Professor Barlow, and said that they differed from those of Lamé, Rankine, and Hart considerably. No doubt they did differ considerably. He might state that his father's attention had been drawn to that subject not with reference to guns, but mainly in reference to Bramah presses. In his idea he had arrived at conclusions considerably greater than those come to by Lamé and Rankine as to the relation between the average stress upon the metal and the internal stress. Fortunately the departure that he made from what was now called the true theory was on the safe side, and the result was that the Bramah presses constructed upon his principle were perfectly safe and good, none ever failed, and that brought his formula into very great popularity. He was quite prepared to admit that the resulting value was in excess of what was necessary for a gun, and in that respect Lamé, Rankine, and Hart were more correct. They were, however, subject to a modification which had been pointed out by Dr. Hopkinson, and which tended backwards a little way towards what had been done by his father. A further modification was really required in investigating the gun, if the ring or cylinder which was called upon to bear the circumferential strain was also called upon to bear the longitudinal strain. In the case of the gun just described by the Author, that point did not arise, for he was not calling upon his wire casing to bear any part of the longitudinal stress of the gun. The theory of Lamé and Rankine was limited to the elastic action of the material. It was said in the Paper that Colonel Rosset considered accordingly that the greatest internal pressure should be that which lay between the extreme limit of elasticity in compression and the extreme limit of elasticity in tension. No doubt, acting upon the limited lines of the theory, he was quite correct. The Author thought that the limit of compression might be very largely exceeded, and spoke of a tube of cast iron which was compressed $\frac{1}{25}$th inch in diameter, whilst the limit of elasticity of the material would be reached with a reduction of $\frac{1}{100}$th inch of its diameter. That meant that it was com-

Mr. Barlow. pressed to sixteen times its limit of elasticity in compression, and that appeared to be rather a large excess. But while the Author thought that this very large excess might be allowed in compression, he seemed to consider that no excess should be allowed in tension. He did qualify that opinion to a certain extent at p. 84 of the Paper, where, speaking of a hoop-gun, and of the case in which there might be a little excess of strain upon the outer portion of the gun, he said, "Now if it should so happen that the excess strain causes a permanent set just equal to the original excess shrinkage, it is clear that the gun has been put right, that is to say, into the condition under which it ought to have been built. Subsequent firing will not alter these conditions, for the simple reason that the strain of explosion no longer exceeds the elastic limit." He quite concurred with the Author in that, and thought it was a very sensible view. Sir William Armstrong, in speaking upon this subject some years ago, said, as the result of his experience, that very strict mathematical rules need not be adhered to; and he was not speaking there from any theory upon the subject, but was giving the result of a long experience in gun-making. Although a slight departure might be made from the strict mathematical condition, yet there was no doubt that the nearer these could be approached the better, and under those circumstances the members were entitled to give their particular thanks to the Author of the Paper, and to Mr. Brooks, who had assisted a good deal in the mathematical department, for the elaborate working out which they had given to the various cases put in the Paper of different actions of wire in the coating of guns.

There was one point, however, in the Paper which he should like to know more about. The wire was said to be of a quality which could be obtained to bear a stress of 100 tons to the inch before it broke; but the Paper implied that 20 tons per inch was the limit of elasticity of that wire.

Mr. Longridge. Mr. LONGRIDGE replied that he believed the elastic limit of such wire as would be used was about 20 tons, to which he limited every strain in the calculations he had made.

Mr. Barlow. Mr. BARLOW said experiments had been made upon this wire at the Arsenal, and although there were two cases in which a very low elastic limit had been displayed yet in the majority of the results the elastic limit was about 50 tons.

Mr. Longridge. Mr. LONGRIDGE observed that in that case he could make his limit 40 or 50 tons as the case might be.

Mr. Barlow. Mr. BARLOW said of course that had its influence upon some of the observations made upon those designs. It varied of course very much in different qualities of wire; but he should say that any wire possessing a strength of 100 tons to the inch, which **Mr. Barlow.** showed anything like permanent set at 20 tons, was wire in an imperfect state of manufacture, and might by a little treatment be made into a wire of the sort he described. The Paper did not entertain the question of the thickness of the wire, and in that question was involved the extra strains that came upon the exterior of the wire compared with those which came on the interior. He had one word to say upon the subject of ballistics. The conditions put in the Paper were said to be calculated upon investigations made by Sir Frederick Abel and Captain Noble. He had not endeavoured to follow them out, but had compared the results in the Paper with what were known to be the results of chambered guns. A 13-inch chambered gun, something of the type shown in Fig. 21, with regard to its interior ring ought to develop about 30,000 foot-tons of energy with a pressure of 18 tons on the interior of the gun. There must be some cause for the difference between that and the results recorded in the Paper. He might mention examples of guns made at the Arsenal, or guns constructed by Sir William Armstrong. Comparing, for example, the energy with the cube of the diameter, which he found a convenient measure, the quantity amounted in one case to more than fourteen times the cube of the diameter, the cube of the diameter being given in inches, the energy in tons per foot, and that was with a pressure of 16½ tons per square inch. In another case the same thing amounted to fifteen times the cube of the diameter with a pressure of 18½ tons per square inch. The third case was that of Sir Joseph Whitworth's gun, weighing 20 tons—a chambered gun. The energy developed 9,786 tons, being 14·8 times the cube of the diameter. In another experiment with a 400-lb. projectile and 194 lbs. of powder, he got 16·7 the cube of the diameter; and in fact it might be taken that in well-proportioned chambered guns the energy in foot-tons was about equal to 15 times the cube of the diameter in inches, with a pressure not exceeding 18 tons per inch in the chamber. Of course the weight of a gun, so far as the weight required for strength, depended upon its pressure. In this particular case it was felt to be necessary to add weight to the gun beyond what was required for its strength; for he understood the Author added a cast-iron casing, which was in fact adding weight, the function of which was to diminish the recoil.

One word about the powder. There was a very large variety in the relation of the energy produced by a powder to the pressure in the gun; and looking through the various trials which he had remarked this relation differed as much as 50 per cent., depending upon the quality of the powder used in the same gun. The Author

Mr. Barlow. observed that slow-burning powder required larger charges and was not economical. That was quite true, but it produced apparently very good results as regarded the gun. The want of economy in the powder was not an important matter in respect to money. It was not important in fortresses where the powder could be easily stowed; it might become important in ships, and was certainly very important in field artillery where the powder had to be carried with the guns. That subject required and was receiving great attention. Then, for the interior of the tube the Author suggested cast-iron. Mr. Barlow did not think that cast iron would be capable of sustaining the effects of erosion which resulted from the firing of gunpowder. He had an opportunity of seeing casts taken from the mortars engaged in the bombardment of Sveaborg. Those guns had fired some one hundred and fifty rounds, and the result of erosion was such that after that they seemed to be capable of doing very little more. Other guns had stood five hundred rounds. He did not know that he need say more, excepting this; there had been, on the whole, a very great improvement some years back in artillery. He measured that improvement in this kind of way; namely, the energy developed by the gun as compared with the weight of the gun. Going back to the days of the old Woolwich Infant, about 350 to 450 tons of energy could be got out of 1 ton of gun. At the next stage, with breechloaders and somewhat longer guns, 450 to 550 tons of energy per ton of gun were reached. Then steel-hooped guns gave 550 to 600 tons of energy; and the wire guns, of which he had a very great opinion, would no doubt develop a higher state of things than that. It was quite true that as those energies were increased in proportion to the weight of the gun, they increased to a certain extent the difficulty of dealing with the recoil. But there were many excellent contrivances for that purpose; it was a separate study, and had been carried out in many ways, and he believed in time it would be very well done.

Sir Frederick Bramwell. Sir FREDERICK BRAMWELL, Vice-President, said he did not know that he could do better than follow his colleague, Mr. Barlow. It so happened that both of them, independently, had had their thoughts directed to the same two subjects, viz., the wire and the chambering. With respect to the reinforcing a tube by successive layers of wire or riband, he had in that Institution, many years ago, and elsewhere, frequently expressed the opinion that the system appeared to possess promises of success, such as entitled it to have the experiment tried upon a large and practical scale. That he was happy to know, was now being done in more places

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than one. Subject to one remark he would now leave the subject of the structure of the gun. The one remark he wished to make was this. As he followed the reading of the Paper, the Author, in all cases in his calculations, dealt with the wire or riband, as though either it were infinitely thin, or as though it were wound round about a tube of infinite radius; that was to say, the Author in every case treated the tensions upon the wire as though the wire were straight, and as though the tensions were not complicated by the convolutions of the wire round about a cylinder. Mr. Barlow just touched upon that point, and then left it, but Sir Frederick Bramwell wished to call attention to this, that in a by no means improbable case, viz., that of a 6-inch gun, having a tube 2 inches thick, giving 10 inches outside diameter, if the riband was $\frac{1}{16}$ inch thick, the extension of that riband on the outside beyond its neutral axis was $\frac{1}{160}$ part. If it were the fact that the wire, such as was used for wire-guns, extended $\frac{1}{8,000}$ part of its length for every ton tension per square inch, it followed, that if there was an extension of $\frac{1}{160}$ part of its length, it amounted to an extension of 50 of these 8,000ths, and thereupon the whole of the Author's very delicate calculations were, as it appeared to him, neutralized by the fact he had neglected that the mere winding of a $\frac{1}{16}$-in. wire about a 10-inch cylinder, put on the outside of that wire, even if it were wound on with no retarding strain at all, a tension of 50 tons to the square inch; and that seemed to point to this, that the necessity of attending to the elastic limit could not be so great as the Author suggested it was; for if it were, the whole of these wires would be absolutely useless, in consequence, not of the tension at which they were put on, but of the mere tension which arose in bending them about a 10-inch cylinder. Further the very mode of manufacture of wire showed that it could not be crippled in the way in which the Author suggested, by passing the elastic limit. Consider in what way wire is manufactured? The nippers which lay hold of the end of the wire, are attached to the circumference of a drum, the thinned-down end of the wire is passed through the draw-hole, technically called the "triplet"; the end is attached to the nippers, and then the drum is set revolving. The drum draws the wire in a tangent, to the circle of the drum, it is true, but it is immediately coiled upon the drum under the strain that is requisite to bring it through the triplet; and thus, in the very outset, it must receive a bending, under the tension, which ought to destroy the integrity of the wire, if the elastic limit were such a critical point as was suggested. Practice showed that this could not

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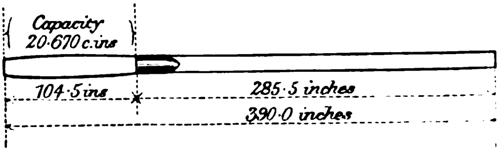
be so, because there were now a great many wire guns made, and he could not help thinking, that in some occult way, the evils which the Author predicted must arise, if the elastic limit were passed, were not produced, for if they were, it would be impossible to make an efficient gun, upon the Author's own system, a conclusion at which Sir Frederick Bramwell would be very sorry to arrive. The other point upon which he wished to speak was the question of chambering. This was treated at p. 105 of the Paper where the Author contrasted the ballistics of a chambered gun, with those of an unchambered gun of equal diameter, and within 4 inches of the same length, the chambered gun using 500 lbs. of powder, and the unchambered gun 413 lbs. The result at which the Author arrived, was that the chambered gun would give a total energy of 17,317 foot-tons, while the unchambered gun, with 87 lbs. less of powder, would give a total energy of 30,103 foot-tons. That was an excess of more than 12,000 foot-tons. He believed that this conclusion at which the Author had arrived was due to an error in his calculation. He would say unfeignedly, that if he found himself differing from the Author in a matter of calculation, he believed it but too commonly arose from this: that the Author was right and he wrong. But in this particular instance, although he had the fear of this solution of their differences before him, he could not help suggesting, the Author had made a mistake. He would endeavour to show at all events, why he thought the Author was wrong. He would ask the members to be good enough to look at three diagrams, Figs. 33, 34, and 35. Fig. 33 showed roughly the Armstrong chambered gun. The length of the chamber was 104·5 inches; the length of the bore beyond the chamber 285·5 inches; making the total length 390 inches, the capacity of the chamber being, as before stated, 20,670 cubic inches. The Author said that chamber was equal, and Sir Frederick Bramwell agreed with him, to a length of the gun with a 13-inch bore of 155·76. On Fig. 34 he had lengthened the gun the difference between 155·76 and 104·5, viz., 51·26, so as to give in an unchambered 13-inch gun, the same capacity of the powder

FIG. 33.



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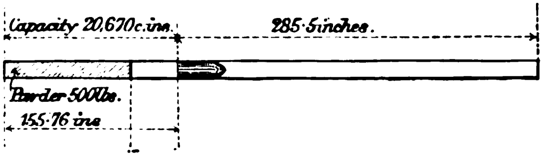
chamber, viz., 20,670 inches, as was shown in the chambered gun. He had repeated the Author's calculation upon the diagram, and it was as follows:—

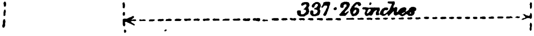
FIG. 34.



$$1 + \frac{\overset{\text{Length of Bore.}}{390\text{ inches.}} - \overset{\text{Length of Chamber.}}{104\cdot5\text{ inches.}}}{\underset{\substack{\text{Length of 13-inch bore equal to}\\ \text{capacity of powder-chamber.}}}{155\cdot76\text{ inches.}}} = 2\cdot832\text{ number of expansions.}$$

Then the Author took from Noble and Abel's Paper, that with 2·832 expansions the ballistic effect was 66·386 foot-tons per pound of powder, and from that he deducted the value of the expansion 1·42; that was to say the value of the expansion of the powder from the space it occupied at a gravimetric density of 1 into the contents of the powder-chamber; this value was 28·348 foot-tons per lb. of powder, and deducting that from 66·386 it left 38·038, the total work in foot-tons for 1 lb. of powder, so that for 500 lbs. he obtained 19,019 foot-tons as the total work of the gun. Now he did think that the Author had made a mistake, and that he would find he had in effect deducted the loss by the expansion 1·42, twice over, once in the outset in arriving at his number of expansions, and the second time

FIG. 35.



$$1 + \frac{\overset{\substack{\text{Length of Gun from}\\ \text{end of Powder.}}}{337\cdot26\text{ inches.}}}{\underset{\text{Length of Powder.}}{104\text{ inches}}} = 4\cdot2\text{ number of expansions.}$$

Sir Frederick Bramwell.

as a deduction from the effect of that number of expansions. He would ask the meeting to consider the diagram, Fig. 35, which he put forward as showing the mode in which in his judgment the expansion ought to be calculated. This as would be seen gave an expansion of 4·2 times. The value of 4·2 was 84·176 foot-tons per lb. of powder, from which, deducting the 8·348 there remained 55·828, as the value in foot-tons for each lb. of powder, as the factor, which multiplied by 500 lbs. gave as the value of that gun a total energy of 27,914, or in round numbers 28,000 foot-tons, in lieu of the Author's 19,000. Mr. Barlow had shown that the chambered guns actually made of that size had 30,000 foot-tons total energy; Sir Frederick Bramwell arrived at 28,000, while the Author estimated the effect at only 19,000. He did really in this instance believe that the Author was wrong and that he was right, and he thought he had pointed out the nature of the mistake. The Author had treated his own gun similarly unfavourably, but as he proposed to use powder at a gravimetric density of $\frac{9}{10}$, instead of the gravimetric density of somewhere about $\frac{7}{10}$, the error of his mode of treatment did not make anything like the same difference in his gun as it made in this, and therefore if Sir Frederick Bramwell was right it altered very much indeed his comparison of 19,000 tons in the one case as against 28,000 in the other.

Mr. Atkinson.

Mr. W. Atkinson said Mr. Barlow had referred to experiments on the strength of wire, and he assumed that an extensive series had been tried. The late Sir William Siemens had tried some experiments on the elastic limit of steel wire, and had come to the conclusion that it was capable of a kind of education, namely, if wire that would break with a weight 1 was subjected to something less than the weight 1, and this was allowed to remain a considerable time, then a weight which exceeded 1 might be put on, and so on until the wire at last would bear a considerably higher strain than at first. He did not know whether the Author could give some information upon that point, because it seemed to bear in a very important manner upon the question of the building up of guns by wire under continual tension. If the Author constructed his gun with the wire under tension, and that wire acquired additional strength by being under that condition for a time, in a short period the whole relation of the gun would be changed.

Mr. Barlow.

Mr. Barlow said a paragraph in the Paper bore strictly upon the point just mentioned. It was this, "Colonel Rosset states, that up to a certain point the elongations were proportional to the

Mr. Barlow.

stresses, but beyond that, they increased rapidly, and in a varying proportion, and that when in excess of the elastic limit, permanent elongations remain after the strains are removed." That was the first effect; it was followed by this, "when the bar is again submitted to tension it is found that the permanent elongations do not begin so soon as before, and that consequently the elastic limit of the material has been increased"—that was the tension of which Mr. Atkinson spoke—" though the modulus of elasticity remains practically the same." Therefore the condition of putting what might be termed a permanent set of moderate extent was that the elastic limit of the material was increased and the modulus of elasticity remained the same, and from his observations he thought Colonel Rosset was quite right on that point.

Mr. Preece.

Mr. W. H. Preece thought he could speak with some little knowledge of the behaviour of wire under different circumstances and under different stresses. He had had occasion at different times to examine the quality and condition of wire that had been submitted to tensions for a good many years. Not very long ago some wire that had been erected forty years was subjected to very careful examination, and it was found that although the wire had been exposed to the atmosphere during all that time, under considerable stress, it retained precisely the same strength that it possessed when it was first erected. Of course that wire had been subjected to very different conditions and to very different stresses to those wires dealt with in the Paper. He did not pretend to know anything about guns, but he did know a little about wires, and the way in which wire conducted itself under different conditions, and he should like to ask the Author one or two questions, because he was bound to confess that a careful listening to the Paper had not convinced him that wire as at present produced was competent to withstand the stresses to which a gun was subjected. The Author suggested that the wire should be laid in coils round the gun under very severe tension, a tension that closely approached the limit of elasticity of the wire; and after the wire had been subjected to this tension it had to undergo some rather rough usage. It had to be enfolded in a cast-iron casing, it had to be subjected to heat, and it had to undergo considerable molecular change. Steel wire was in existence that would stand a stress of 100 tons on the square inch, but if that wire was subjected to heat, or to any annealing process, its tensile strength would be very much diminished, and he should expect to find that if wire with a tensile strength of 100 tons per square inch was subjected to the rough usage of thermal and

Mr. Preece. mechanical appliances, its original strength would be very considerably diminished. The Author and Mr. Brooks in their calculation had concluded that the wire retained its original strength, but Mr. Preece doubted very much whether that would be the case. The wire was coiled around the gun, and was subject to a very considerable tensile force in the direction of its length. The wire was coiled around the core, and by taking one portion of one spiral each coil might be regarded as a ring. Suppose a sudden explosion of gunpowder to occur in the chamber exerting a pressure of 30 tons on the square inch, then the effect of a pressure of 30 tons on the square inch would be to distort that ring, and, in fact, to form a ring of greater diameter. Now if in the process of construction that wire had been formed into a ring with a tension closely approaching its limit of elasticity, he should assume that when it was subjected to a pressure of 30 tons on the square inch, in which it would be subject to a distortion which would be practically an elongation, the wire would then be under a strain exceeding its tensile strength and burst. The Author had spoken of the influence of drawing upon the wire. He said, "Let now this wire be drawn down to half its diameter, it will bear at least the same strain per square inch, although it has undergone a very large change of form." That was perfectly true; the wire, in being drawn down, not only retained its strength per square inch, but absolutely had its strength increased. But the Author added, "Mere change of form does not necessarily imply reduction of strength; in fact, in the case of wire-drawing it considerably increases it, and the action of the normal forces on a wire in a gun-coil is simply a case of wire-drawing." He was bound to confess that he must join conclusions with the Author on that point. Such an action as the simple distension of the coil was not at all analogous to the action involved in wire-drawing as explained by Sir Frederick Bramwell. There the wire was drawn through a hole, and in the process of drawing through the hole there was a considerable compression of the molecules, and the result was a considerable accession of strength. Under mere extension, as in breaking a wire, there was a reduction in the diameter without a compensating effect of a compression of molecules, and the result was the wire got weaker and weaker, while in the act of drawing it got stronger and stronger.

Mr. Beaumont. Mr. W. W. **Beaumont** wished to direct attention to the credit due to Dr. Hart with reference to the fundamental formula with which the stresses in ring-structures were obtained. He noticed that several names besides that of Dr. Hart were mentioned, Mr. Beaumont.

but one name well known in the Institution, viz., that of the late Mr. Robert Mallet, M. Inst. C.E., had not been mentioned; and it was only due to Mr. Mallet to state his belief that he was the first to make use of and really complete for Dr. Hart that mathematical investigation, which would not have been brought out but for him.[1] Sir Frederick Bramwell had mentioned the difference in the various laminæ which might be used in any thickness of wire when bent and subjected to a stress such as that to which that wire would be, and he had mentioned that the Author had treated the compression of the wire in the gun as though the wire were of infinite thinness. He believed on the theorem of Lagrange which he had mentioned in a Paper read before the Institution,[2] it would be found that the difference in the stresses throughout the different parts of a section of wire so bent could be found. Sir William Armstrong had pointed out at the Southampton meeting of the British Association[3] that it was necessary to take into consideration the fact that very much greater tensile stresses would be thrown upon the exterior layer of a wire, unless of infinite thinness, than upon the inner one; and it had been suggested since then the wire might be drawn in such a way that, when wound round the gun, it should have the same tension throughout the whole of its section. It was proposed that this should be done by drawing the wire, as a last process, through a die, which should consist in the upper part of a rubbing die in the ordinary way, and in the lower part of a wheel; the wire, supposing it to be a riband wire as was chosen by Sir William Armstrong, would then pass over the roller and would not on that side have its texture in any way altered; while the upper part passing under pressure through the fixed die would be compressed, or rather, it would be elongated. Of course it would be necessary to cause the wire to come from that die nearly of the same diameter that it would afterwards have to take when coiled on the gun. The Author, referring to a 3-inch gun made some time ago, said that the interior of the tube of that gun was cast-iron, and he mentioned that during the coiling of the wire the core decreased in diameter $\frac{1}{25}$ inch. He remarked that that was very much greater than the elastic compression of the material, and came to the conclusion that in the form of a

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[1] "On the Physical Conditions involved in the Construction of Artillery," 1856.

[2] Minutes of Proceedings Inst. C.E., vol. xlvii., p. 45.

[3] Report, 1882, p. 398.

Mr. Beaumont. tube the cast-iron or any material might be compressed much more than its elastic compression. With reference to that point he should like to ask if, when that gun was fired, the compressive stress, which resulted in decreasing the diameter of the tube, continued. It would seem the decrease in the diameter to that extent indicated destructive compression of the material, because, as far as he knew, although the Author mentioned that he supposed the decrease in the diameter of the tube was due to the condensation of the iron, he did not think it had been found that iron was really compressed in that way. He believed when cylinders were subjected to a change of form in that way their specific gravity was not altered; and, if that was the case, then the compression in the tube must be immense, and when the gun was fired it might have been expected that the decrease in the diameter would be considerable. The material of the tube being free to flow inwards, compressive stress greater than that necessary to cause deformation would be dissipated until the stress fell to near about the elastic limit of the materials. Perhaps the Author would state whether that was so or not. The Author had quoted Clavarino, and said: "Clavarino has put forth an opinion that the strength of a gun is limited not by the tension of the material, but by its extension. It amounts in fact to this, that the extension, say of a wire, by the joint action of the longitudinal and the lateral strains, fixes the limit of strain which the wire can resist. He says: 'If the elongation (caused by the three forces) equals that which is produced at the limit of elasticity by simple traction, then the prism is on the point of undergoing a change of form, although the force acting in the direction of elongation may not equal the resistance at the elastic limit.'" The Author objected to that paragraph, thinking it altogether at variance with the facts, but why he objected to it was not apparent, nor was it at all clear what was meant by the paragraph previous to the quotation. He should like to ask the Author what he intended by that paragraph. Clavarino's statement was to the effect that permanent elongation might take place under orthogonal stresses, though the stress in direction of elongation was below the elastic limit.

Mr. Baker. Mr. B. Baker said there was one little point of difference between the Author and Sir Frederick Bramwell that might be reconciled, and that was with reference to the part of the Paper where the Author assumed the modulus of elasticity of the wire in all these calculations to be 22,000 tons. Sir Frederick Bramwell's calculations of the stresses resulting from coiling the wire *implied* a *modulus* of 8,000 tons, or a difference of $2\frac{3}{4}$ to 1. The effect would be that Sir Frederick Bramwell's 50 tons, which he considered due to bending the wire, would be brought up to 130 tons. The elastic limit would thus be considerably exceeded, unless indeed the wire were of the highest quality and had been previously stretched. He should like to ask where the Author had got wire with 22,000 tons modulus. He thought that about 12,000 to 13,000 tons would be a happy medium in this case. The question was a small one and did not affect the theory of wire-guns, though it did affect the Author's diagrams and numerical results. Mr. Baker.

Mr. J. A. Longridge, in reply, said, speaking of the question of gunpowder, which had been referred to by Colonel Maitland, his argument throughout the Paper had been that powder-makers had been retrograding in the manufacture of gunpowder for the last twenty-five or thirty years. They had been making weak gunpowder to suit weak guns, and as corroborative of that he read a paragraph from Colonel Brackenbury's recent Paper which supported his view. Colonel Maitland said if strong powder was required why not take gun-cotton. That was exactly the question which he put to Colonel Maitland about two hours previously, when he asked whether the authorities ought not to try to moderate the action of gun-cotton instead of trying to weaken that of gunpowder. He mentioned to him that Baron Lenk many years ago in Austria had, as he was informed, succeeded in regulating the action of gun-cotton not only for small arms but for artillery. How far he had succeeded he could not pretend to say, but he was quite sure that if it could be done, gun-cotton would be far superior to gunpowder, not only in being less bulky, but also in avoiding that dreadful nuisance of smoke. How it would be possible to carry on naval actions now that it was proposed to have charges of powder equal to half the weight of the shot, he did not know; how, looking at the nuisance of smoke, the enemy were to be seen, he could not say; but irrespective of the question of smoke, he felt sure that if gun-cotton or any other material could be produced stronger instead of weaker than gunpowder, all that was necessary was to try to make the guns strong enough to govern it. That he was certain could be done. He was sure that by the application of wire he could make a gun which would resist an internal strain of 50 to 60 tons per square inch, which should be as safe as the present guns with an internal strain of 15 tons per square inch. The investigations which he had carried out would, he thought, prove that to Mr. Longridge.

Mr. Longridge. demonstration. It remained with those who had the power, such as the great establishments at Woolwich, at Elswick, or elsewhere, to experiment, not on a tentative process, but upon the lines now set before them on strict mathematical evidence. If any gunmaker was disposed to consider the question, Mr. Longridge's services were at his complete and free disposal. He should be most happy to give every information either to those at Woolwich or Elswick, or to Mr. Vavasseur, M. Inst. C.E., or any one else who would seriously go into the question, and he felt sure that if one-thousandth part of the money which had been spent in other experiments had been applied to wire-gun experiments, wire guns would have been in a very different position from what they now were. In his Paper he referred to the "Daring" gun. Colonel Maitland said that many guns of the same nature as the "Daring" had fired hundreds of rounds, and would continue to do so. That was perfectly true, and Mr. Longridge had said as much in the Paper; he said the gun might fire hundreds of rounds, but it was beginning to fail from the commencement, and sooner or later it would give way, and to that opinion he still held. How was it if these guns were perfectly safe that, immediately the accident took place, orders had been issued that they were only to be fired with reduced charges? He was very happy to hear from Mr. Barlow that wire could be got with an elastic limit of 50 tons. He had not been aware of that, and therefore preferred to be well within the mark, and in all his calculations to take it as 20 tons. If 50 could be had so much the better for his wire gun. Some remarks had been made by Mr. Barlow and Sir Frederick Bramwell about the strains induced in the wire by the act of coiling it about a core, and the same question had previously been raised by Sir William Armstrong at the Southampton meeting of the British Association. This was a point which the Author had not lost sight of, but being of minor importance, and considering the length of the Paper, he had not referred to it therein. Sir Frederick Bramwell, taking the case of a 10-inch core wrapped round with $\frac{1}{16}$-inch wire, argued, that if the wire were put on with no tension, it would, by the act of coiling on, be strained to 50 tons per square inch at the outer circumference. This would probably be true if two conditions existed; first, that the limit of elasticity was equal to or exceeded 50 tons per square inch, and second, that the wire, previous to laying on, was a straight wire of indefinite length. In reality it was not so. The wire as used was already in the form of a coil, and in that state was free from internal strain. Were it

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otherwise, were it in tension on one side of the neutral axis, and consequently in compression on the other side, it would necessarily straighten itself. The extra strain brought on in coiling on the gun, was only that due to the difference of the radius of the drum on which the wire was coiled, and that of the coil when upon the core, and this might be manipulated indefinitely. This question would be found dealt with more at length in the Author's forthcoming work; at present he would only add that the extra strain from coiling did not give rise to any difficulty in the construction of the guns. He should not think in a 6-inch gun of putting on a $\frac{1}{16}$-inch wire for the inner coils, but he should prefer $\frac{1}{40}$-inch wire. Even with a $\frac{1}{16}$-inch wire the strain did not exist to the extent that Sir Frederick Bramwell thought. This very question of special elasticity which Colonel Rosset drew attention to, and which it was very desirable should be thoroughly investigated, showed that the wire was permanently stretched, but it did not retain the force with which it was stretched; it could at the very most retain the stretch of its elastic limit. The question had never been lost sight of, but it practically amounted to such a very small matter, that it was no real objection in the case of large guns. As to the question of erosion, rifled cast-iron guns were things of every day. The French artillery consisted almost entirely of rifled cast-iron guns. It was true the French were making some exceptional special guns, in which they did not seem to have much confidence, with steel tubes, but the great bulk of the artillery was cast-iron. So also was the American and Italian. The question of erosion was embarrassing with these large charges, on account of the enormous amount of powder which, being to a great extent an inert material, formed a fluid mass, and rushed along the gun. No doubt erosion was an inconvenience, and must diminish to a certain extent the life of the gun, but he would rather take care of the life of the men than the life of the gun. If the gun got unserviceable, nothing could be easier than to retube it, but he believed that erosion could be very greatly done away with by means of gas-checks; at any rate, he maintained that the question of erosion was quite beside the mark, inasmuch as nearly every other country was using cast-iron for the inside. Mr. Barlow, whilst not challenging the Author's calculation of the ballistic effort of Sir William Armstrong's 13-inch gun (Fig. 21), spoke of comparing the results with what he said was known to be the results of chambered guns. He said a 13-inch chambered gun, something of the type shown in Fig. 21, ought to develop about 30,000 foot-tons of energy with a

Mr. Longridge. pressure of 18 tons on the interior of the gun; but he did not say that it had actually done so. It was not possible for the Author to argue against what a gun ought to do, nor could he say what it really would do, as Mr. Barlow did not give the weight of the charge of powder; but he had no hesitation in saying, that with a charge of 500 lbs. of powder in the chamber of the gun, Fig. 21, it was impossible for anything like 30,000 tons energy to be developed. He confessed that he could not follow Mr. Barlow's comparison between the energy and the cube of the diameter of the bore. He could not conceive the relation that there was between them in any possible way, and therefore could draw no conclusion from the figures which Mr. Barlow had submitted. Neither did he think that the proportion between the energy and the weight of a gun was a matter worthy of consideration. He could make his gun as light as he chose. If the recoil could be done away with he would reduce the weight of the gun as much as was desired. It was simply in order to avoid the inconvenience of recoil that he gave that weight, but he gave it with a material costing about £5 a ton instead of the enormously expensive steel and iron which was put on to such guns as that represented in Fig. 21. Mr. Barlow had remarked, that economy of powder was not a very important matter with regard to money. Perhaps not, although in firing large guns with projectiles weighing 1,000 lbs. the difference between 200 lbs. of powder and 500 lbs. would amount to a pretty large sum; but there were other considerations, and amongst them the amount of smoke which such enormous charges would give out, the inconvenience of such large cartridges, and the difficulty of transport and storage. Mr. Barlow had again referred to Professor Barlow's theory, and admitted that it differed from the true theory as given by Messrs. Brooks, Lamé, Rankine, and Hart, who had all arrived, independently of each other, at the same formula. He also remarked that Sir William Armstrong said, as the result of his experience, strict mathematical rules should not be adhered to, and that this was not speaking from any theory, but was the result of a very long experience in gun-making, and Mr. Barlow referred to a passage in the Paper, as to some extent supporting this view. Now the passage in question simply referred to a possibly exceptional case, in which by chance the permanent set was exactly equal to the excess error in tension, and must not be taken as in any way supporting Sir William Armstrong's contention. Coming to Sir Frederick Bramwell's calculation with respect to these two guns, he would give what he had said his most careful considera-

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tion; and if he found that Sir Frederick Bramwell was right, as he very often was, nobody would have greater pleasure than he in correcting the matter. All he could say at present was that his calculation followed exactly in the lines given by Captain Noble and Sir Frederick Abel, in which they took a precisely similar case, and worked it out according to their Tables. He followed exactly on their method, and did not pay particular attention to it, thinking that such high authorities as they were must be right. Mr. Preece appeared to have misunderstood the Paper. He said that the Author suggested that the wire should be laid on in coils under very severe tension, closely approaching the limit of elasticity of the wire. The object of the Paper was, on the contrary, to determine the laying-on tension, so that when supplemented by the strain from the powder-pressure, it should never exceed the elastic limit. The diagrams in the Paper showing the conditions of a gun when the wire was laid on with a high initial tension, were placed there expressly to show the pernicious effect of a high laying-on tension, and not to show how a gun might be made. Mr. Preece had fallen into another mistake in supposing that after the wire had been put on it was subjected to rough usage. He said it had to be encased in a cast-iron casing and subjected to heat. The operation of putting the coil into the outer case was very simple, it involved no rough usage, and the coil was not subjected to any heat. With reference to the action of compression upon the small 3-inch gun, he had been asked by Mr. W. W. Beaumont if he measured the gun after it had been fired. He had not done so. It had been fired a great many times, and was apparently as perfect now as ever it was. There were no signs of erosion or fracture, or cracks, or anything of the sort. Mr. Beaumont was mistaken in thinking that cylinders when subjected to change of form in that way, did not alter their specific gravity. They did alter their specific gravity. Mr. Beaumont had reproached the Author with having omitted Mr. Robert Mallet's name. The Author did not mention Mr. Mallet because he was speaking of those who had investigated this question mathematically, and Mr. Mallet in his book expressly stated that it was Dr. Hart who made the investigations for him. As to the modulus of elasticity which he had taken, which Mr. Baker stated differed widely from that assumed by Sir Frederick Bramwell, he did not go very particularly into what really was the modulus of elasticity of the best steel wire, for it was beyond his personal knowledge. He had taken those figures from various documents in his possession, chiefly from the U.S.A. Ordnance

Mr. Longridge. reports, which contained a great number of experiments upon that subject. Steel wire no doubt had been and could be obtained with a modulus of 22,000 tons, but that did not at all affect the general question; it merely affected the value of the "*c*," which was one of the constants introduced into the formula. The formula was in such a general form that any modulus of elasticity, any radius of core or coil, and any dimension of gun, could be adopted. It would give the exact strain with which to lay on the wire if a uniform tension under fire were desired; or if uniform tension in laying it on was the object, another formula would give the exact strain under fire and at rest. It was perfectly general in form, and he sincerely hoped that the gun-makers would give it full and fair consideration, and put it to practical proof, instead of continuing to construct wire-guns by a tentative process. It was gratifying after thirty years to find that the views he then expressed as to the value of wire were being recognized; but it was grievous at the same time to believe that what was being done, and had been done up to the present time, had not been guided by theoretical and scientific considerations, but by a tentative process.

Mr. Woods. Mr. EDWARD WOODS, Vice-President, said all would agree that the Author had made a valuable contribution to the science of the construction of guns, and it must be gratifying to him to see that the subject was now receiving attention from those conversant with the details of the manufacture of ordnance. All present would join in the wish that the experiments now being made would be carried out to the Author's satisfaction, and would show the value of the system which he had been advocating.

Correspondence.

Mr. Longridge. Mr. LONGRIDGE observed that Sir Frederick Bramwell had taken exception to the calculation at p. 106 of the Paper, which represented the comparative power of the two guns, as obtained from Table XI., according to Noble and Abel's approximate method, detailed at p. 240 "Researches on Expansions;" that was to say,

	Foot-tons.
Chambered gun, 500 lbs. powder	17,357
Unchambered " 413 " "	30,103

Sir Frederick Bramwell had challenged these figures, and found by his own method of using the Tables, an energy for the chambered gun with 500 lbs. = 26,357 foot-tons. The Author,

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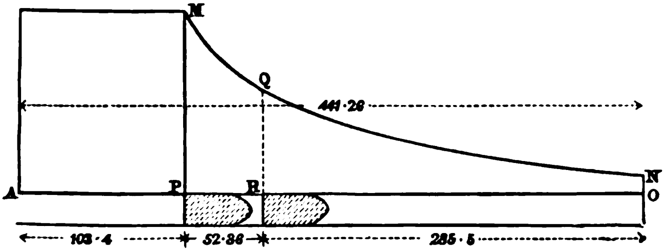
having examined the method of Messrs. Noble and Abel, had come to the conclusion that, as applied to this gun, it did not give even approximately correct results; but at the same time he could not admit the correctness of Sir Frederick Bramwell's method. It was admitted by Sir Frederick Bramwell that the chambered gun was equivalent to an unchambered gun, lengthened by the difference between the actual length of the powder-chamber, and a length of 13-inch bore of equal capacity, say by 52·36 inches, as shown in Fig. 36.

He thus assumed 500 lbs. powder placed in the 103·4 inches, gravimetric density = 1, and the shot at P, and fired in a gun whose total length was 441·26 inches, or with 4·2 expansions.

Then, from Table XI. "Researches on Explosives," he got: work done per lb. of powder, 4·2 expansion, 84·70 foot-tons. Next he assumed the gun shortened to R, or 155·76 inches long, and fired with a similar charge, and shot in the same position, which gave 1·5 expansion, and from the Table he obtained: work done per lb. of powder, 1·5 expansion, 31·986 foot-tons. Deducting this from the first gave the work done, represented by the area B Q N O, and this he assumed to be the same as the work done by a similar charge fired in the space A R, density 0·6652, with the shot placed at R, and the expansion $\frac{441\cdot26}{155\cdot76} = 2\cdot833$.

FIG. 36.



It certainly would seem as if there should be no difference. In the first case there was the work done from R to O by 500 lbs. of powder, placed in A P and expanded to A R, whilst expanding from R to O. In the second there was the same weight of powder exploded in A R, and working by expansion, also from R to O. Were not the two equal? They were not. In the first case, the powder had expanded from P to R doing work; in the second, the expansion was without doing work.

Assuming the initial tension density = 1 to be 43 tons per

Mr. Longridge. square inch, the tension of the guns at R would be in the first case 18·62, and in the second 20·20 tons per square inch. But this was not the whole difference; the expansion curves did not coincide.

They might be worked out by Noble and Abel's formula (30), which might be put in the form

$$p = p_o \left(\frac{0 \cdot 43}{\frac{x}{c} - 0 \cdot 57} \right)^{1 \cdot 074}$$

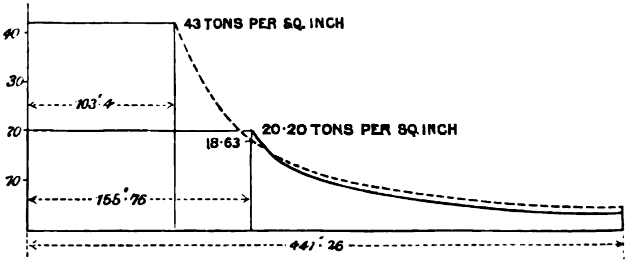
where p_o was the initial pressure,
p the pressure at x,
l the length of the charge.

Now x was always measured from the breech-end of the gun, and therefore, as l was different in the two guns, it was evident that the values of p, corresponding to x, could not be the same in the two guns.

The following diagram (Fig. 37), worked out by this formula, gave the expansion curves in the two cases :—

The dotted line was for the first case, and the full line for the second. It would be seen that, although at 155·76 the full line was above, at about 162 it coincided with, and beyond that it fell below the dotted line. From this it was evident that the work done, as estimated by the first method, was greater than the actual work done, which was given correctly by the second method. If instead of using Table VI. the energy was calculated in both ways from the formula (34), the result would be, by Sir Frederick Bramwell's method of deduction, 52·714 foot-tons per lb., whilst by the direct method it would be only 46·771 foot-tons per lb. The direct method was indisputably the right one, and it there-

Fig. 37.



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fore gave the total energy of the gun with 500 lbs. of powder = 23,385 foot-tons. Proceeding by the same method with the unchambered gun, and 413 lbs. of powder fired at a density $=0 \cdot 90$, it would be found that the work done per lb. of powder was 81·09 foot-tons, whilst for 413 lbs. the total energy was 33,488 foot-tons.

The correct comparison was therefore—

	Foot-tons.
Chambered guns, with 500 lbs. of powder . .	23,345
Unchambered ,, ,, 413 ,, ,, . .	33,488

It must not be concluded that either of these guns would give these results in practice. These results represented the maximum possible effect that powder could give, and they must be multiplied by a fraction which Noble and Abel termed the "factor of effect," and which varied according to the size of the gun and other conditions. They were therefore only to be considered as comparatives, between the chambered and unchambered systems. The comparison was largely in favour of the latter.

Mr. Duval.

Mr. E. Duval, manager of the Fives-Lille Company, was unable to enter into the theoretical discussion of the Paper, the Author not having thought necessary to give the calculations upon which his formulas were based. Had Captain Schultz been alive he would doubtless have had something to say on the subject, but since his death at the end of 1882 the matter had not been pursued in France.

At the same time Mr. Duval would be glad to correct some errors in the Paper relative to the 34-centimetre gun constructed at Fives-Lille. The dimensions of this gun in cross-section were :—

	Millimetres.
Radius of inner surface of core at the powder-chamber . .	185·0
,, outer ,, ,, at the breech	330·0
The thickness of the tube was therefore	135·0
Radius of the outer surface of the coil	443·5
The thickness of the two layers of wire were therefore . .	113·5
The diameter of the wire employed was	3·0

The figures given by the Author for these dimensions, and which had been thus rectified, were taken from the report of the Chief of Ordnance, U.S.A., derived from Mr. Duval did not know what source. Those stated in the same report relative to the coefficient of elasticity of the materials employed were still more erroneous. The conditions of the specification prepared by the French Admiralty were :—

1st.—For the tempered steel forming the interior tube—breaking

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TRIALS of FOUR 24-CENTIMETRE SCHULTZ GUNS.

Number and Description of Gun.	First Round.		Second Round.		Third Round.		Remarks.
	Charge.	Recoil.	Charge.	Recoil.	Charge.	Recoil.	
	Kgs.	Metres.	Kgs.	Metres.	Kgs.	Metres.	
I. Cast iron, not tubed, 7·2 metres long, with trunnion-hoop and breech-plate of steel. Before the trial, marks were made on the head of the bolts, on the nuts, on the breech-hoop, and on the coverings of the wires, to note if any displacement occurred. Also a gauge was established to measure any stretching of the bolts. The gun was placed on a timber cradle made for the purpose	30	3·15	40	5·3	60	8·5	The recoil in the third round was limited by the length of the platform, at the end of which were gabions to arrest the cradle. The shock of the cradle against the gabions sent them back 1 metre. The examination of the piece after each round showed it to be uninjured.
IA. Of similar construction and dimensions. Before firing similar marks were made as in No. I.	30	2·70	40	4·2	60	7·4	The examination of the piece after each round showed it to be uninjured.
II. Of similar construction as No. I. but 6·6 metres long. Before firing similar marks were made as in No. I. . . .	26	2·70	36	4·3	52½	7·0	The examination of the piece after each round showed it to be uninjured.
IIA. Of similar dimensions as No. II., but with breech-plate and trunnion-ring of cast-iron. Before firing similar marks were made as in No. I.	26	2·70	36	4·2	52	7·3	After the first round, an elongation of 2 millimetres was observed on two of the bolts. Also the head of a bolt was found to be depressed on one side to the extent of some hundreths of a millimetre, while on the other side it projected to the same extent. The second round did not affect the bolts already referred to; two other bolts were

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strain, 60 kilograms per square millimetre (38·1 tons per square inch); minimum elongation, 5 per cent.; elastic limit, 30 kilograms per square millimetre (19·05 tons per square inch). The tube employed amply fulfilled these conditions.

2nd.—For the 3-millimetre steel wire—breaking-strain, 190 kilograms per square millimetre (120·65 tons per square inch); corresponding elongation, 2 per cent.; elastic limit, 133 kilograms per square millimetre (84·45 tons per square inch); elastic elongation, 7 per cent.

The several tests made both at the Conservatoire des Arts et Métiers, and at the works of the contractors, gave as the mean:—

Breaking strain	185	kilograms per square millimetre.
Corresponding elongation . .	2·8	per cent.
Limit of elasticity	123	kilograms per square millimetre.
Elastic elongation	7	per cent.

It would be seen that these figures were very different from those given by the Author, and his conclusions would necessarily need modification.

The trials of this gun were, to his great regret, stopped owing to the breaking, at the first discharge, of some bolts uniting the breech-piece to the trunnion hoop. Another gun, however, of 10-centimetres calibre, constructed at the same period for the French Admiralty, of similar materials, had so far (April, 1884) resisted perfectly the long and very severe tests to which it had been subjected during the last two years. He had every reason to believe that the 34-centimetre gun would behave equally well when the modification which had been decided upon had been carried out. This consisted in substituting for the bolts a steel strap. Unfortunately this would take some time.

Mr. Duval submitted some particulars (p. 152) of the trials of four 24-centimetre guns, constructed on Captain Schultz's system. The powder employed was that known as A$\frac{30}{40}$. In each case the Inspecting Committee (*Commission de Reception*) reported that the three rounds had not given rise to any observation of importance worthy to be recorded, and recommended that the guns should be accepted.

Dr. Woodbridge.

Dr. W. E. Woodbridge agreed with the Author that it was important the question of strains should be mathematically considered, and that Mr. Brooks had treated the subject upon correct principles. It was evident, however, that calculations must be applied to ascertained data, and therefore that the experimental determination of the properties of the materials to be used in gun-

Dr Woodbridge. construction lay at the basis of the whole subject. It was of course necessary, too, that all the elements of the problem should be taken into account. From the neglect of one or other of these necessities doubtless arose what were called discrepancies between theory and practice. The calculations of which the results had been presented in the Paper seemed to have been made without reference to the deformation of the wire (whether round or approximately rectangular), resulting from the imperfect contact of surfaces pressed together by the radial strains generated in winding. The effect of the neglect of this element in computation would be an underestimate of the strain upon the interior coils of a gun, as compared with those of the exterior. The extent of this error must be experimentally determined. This was said of guns not soldered. Evidently if the vacuities between the wires were filled with a soldering metal, the several layers would be brought more nearly into the condition of perfect contact. Perhaps a more serious oversight consisted in taking what might be called the working-pressure of the gun as the point at which it was desirable that the strains on the wound wires should be uniform. The highest resistance which the materials were capable of affording was procured when the whole were brought into uniform strain at the maximum strain they were able to withstand. In providing this highest resistance as a safeguard against rupture by abnormal strains, the lesser strains of working pressures were equally well provided for. Inasmuch as steel wire of suitable size might be produced having an elastic limit of 120,000 lbs. per square inch, while retaining an extensibility indicated by the capability of winding around a wire of the diameter of its own diagonal dimensions, the tension of winding might be carried much higher than the Author seemed to approve. True, very high tensions from the cylinder within the coil necessitated very great resistance to the contractile effort of the wire; but this might be advantageously provided by means of the metal serving to give longitudinal strength to the gun. The Author had mentioned four styles of gun-construction in which wire was employed, as having been proposed by Dr. Woodbridge to the late Board on Heavy Ordnance, appointed by Act of Congress. The one placed second on the list was briefly described, with the omission of one or two very important items, in the following words:—" A steel gun, composed of an inner steel tube overlaid with longitudinal bars, and then wound round with steel wire under high tension, and finally soldered." To this it should be added that the longitudinal bars (covering about one-half the

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length of the tube) formed a cylinder of closely fitted staves, and that they were made of cold-wrought steel, having high elastic limits of extension and compression, while they preserved a capability of considerable elongation beyond the elastic limit. This mode of construction differed markedly from those recently essayed elsewhere than in the United States, so far as they had come to Dr. Woodbridge's knowledge, first, in providing a resistance to the constructing force of the wound-wire, so high that the tensile strain upon the latter was in no part changed to compression in winding, and that the interior cylinder was never released from compression, even in firing; and, secondly, by the union of the wires by soldering. The importance attached to the first-mentioned peculiarity arose from the avoidance of the disintegrating tendency of high reversed strains, under such circumstances as existed in this class of construction. One item of the advantages of soldering had already been mentioned, and need not be repeated. In addition to the general solidity, or resistance to displacements which it imparted to the structure, there was, in his view, a specially important feature of this kind. For the purpose of readily presenting this idea, suppose the wire could be frictionless, presenting all the other properties of a suitable gun-wire. Employing this, there would, of course, be no such possibility as varying tensions in a continuous wire, whether in the first layer or in other parts of the coil. But this, whatever might be its result, was impossible. There was, however, a momentary approach to that condition in the firing of an unsoldered wire-gun. The vibratory motions then set up partially freed the wires from the restraints of friction, and they were allowed to "creep" in the direction which tended to relieve them from the heavier strain. This would have no injurious result were the strains uniform throughout the whole length of the helices which enwrapped the tube; that was to say, if pressures were the same in all parts of the bore they surrounded, for then it would amount to nothing more than an equable distribution of strains. The case was, however, far otherwise. The heavy strains near the breech sought to relieve themselves by drawing the wire in that direction, and the original tension was there impaired. This action was, of course, prevented by soldering. A very obvious addition to the advantages of the process was the security with which the ends of the wires were fastened, and the prevention of the consequences of the accidental cutting or breaking of a single wire in an unsoldered gun. Perhaps the citation of an experiment made at the Washington Navy Yard might allay

Dr. Woodbridge. the suspicion of some that the high tensions advocated might not be maintained by the wire, especially when the guns were subjected to vibrations as in firing. A wire 0·15 inch square was extended with a tension of slightly more than 90,000 lbs. per square inch, maintained by a suspended weight of more than 2,000 lbs. Fine marks were placed upon the wire 100 inches apart, and corresponding marks upon a steel rod at its side. A hard-wood hammer, actuated by the power driving the machinery of the shops, delivered blows upon the strained wire at the rate of from four hundred and fifty to five hundred per minute, during working hours, for more than two months. No yielding or elongation could be detected. Two guns of the style mentioned above were now being constructed for the United States Government, and in due time might be expected to furnish the kind of instruction that was after all the most convincing, namely, experimental. It was to be regretted that the Author had not given an account of the trials of his 3-inch gun, to which he alluded in support of his opinion that a cast-iron gun tube might, without disadvantage, be subjected to a compression many times exceeding that within its elastic limit. The Author had referred to the results of experimental wire-gun construction in the United States, and it might be proper that Dr. Woodbridge should mention some facts in that connection. In July 1850 he called the attention of the Ordnance Department of the United States to a plan of construction embodied in a small gun of his invention and production, its linear dimensions being one-third those of the standard 6-pounder field-gun.[1] It was made of flat iron wire having a cross-section of $\frac{1}{8}$ inch by $\frac{1}{4}$ inch, wound upon an iron tube, and brazed. Before being bored to calibre or turned on the exterior, it was fired with a charge consisting of rifle-powder, a wad and leaden shot filling the tube, to make a rough test of its strength, and also for the purpose of expanding its bore so as to bring out the resistance of its metal to an extent likely to prevent further enlargement under ordinary strains. It was finished to a weight little less than three-fourths that of a bronze-gun of the same calibre having the model of the 6-pounder, which was provided for a comparative test. The reduction of weight was made by a proportionate reduction of the thickness of the metal in the different parts of its length. It was subjected to several days' continuous firing, but the experiment relied on as a demonstration

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[1] See Copies of documents given in the Report of the Chief of Ordnance, 1872, p. 159.

Dr. Woodbridge.

of its superior strength was a comparison of the resistance of the two guns to hydrostatic interior pressure. The bronze-gun was ruptured at an indicated pressure of a little more than 32,000 lbs. per square inch. The wire-gun withstood the same pressure without visible change. In 1862 a small steel wire-gun was made at the Navy Yard, Washington, D.C. Nineteen wires $\frac{1}{10}$ inch square were wound at once. The bore was 2·5 inches in diameter, measured on the "lands," or 2·7 inches measured in the grooves, and the exterior diameter 5·4 inches, making the thickness over the grooves just one-half the corresponding diameter of the bore. No lining-tube was used in its construction. The wire was wound on a mandrel of bronze of the same composition as the soldering metal, which commingled with it in the process of soldering and filled the space within the wound wire. The lining of the bore was therefore of the same material with the solder. The weight of the gun, exclusive of the trunnion-hoop, was 149 lbs. The first test of this gun was made under the control of the late Admiral Dahlgren, with charges of 1 lb. of powder, and a 4-lb. projectile having an expanding metallic sabot. The inventor suggested the use of a heavier projectile, but was overruled by the Admiral. This test was carried only to the one hundred and fifth round. A tangential specimen cut from the muzzle of the gun showed a tensile strength of more than 108,000 lbs. per square inch. The gun was subsequently sent to the late Mr. Edwin A. Stevens, of Hoboken, for further trial, the manner and extent being left to his discretion. Mr. Woodbridge had not ascertained the extent of the trial by Mr. Stevens, but upon applying for the removal of the gun for the test afterward made at Springfield, Mr. Stevens informed him that he was "Satisfied that it could not be burst by any charge of powder aud projectile that could be put into it." In 1865, by the direction of the late General Dyer, then Chief of Ordnance, it was submitted to further test at the Springfield Armoury under command of Colonel Laidley. It was there fired thirteen hundred and twenty-seven additional rounds, the charge of powder being constantly 1 lb., and the projectiles either $7\frac{13}{16}$ or $10\frac{3}{16}$ lbs., seven hundred and ninety rounds of the former, and five hundred and thirty-seven of the latter. The following was a portion of Colonel Laidley's report:—"The greatest enlargement of the bore, in the rear of the seat of the shot, caused by the one thousand three hundred and twenty-seven rounds of excessive charges, is only seven-thousandths of an inch, and there are, as yet, no indications of the gun giving way in any part." As a practical test of transverse strength this

Dr. Woodbridge. trial was one of peculiar severity. The brazed wire had no support from a lining-tube, and the cross-section of its metal compared with that of the United States 3-inch wrought-iron rifle, fired with the same charge and weight of projectile, was but as 1 to 3·8. The gun was fired from a 6-pounder carriage of old and heavy pattern, heavily reinforced with iron to give it the necessary strength; the whole weight (probably not less than 1,200 lbs.), practically, as related to strain in recoil, being added to the gun at the trunnions. In accordance with the recommendation of the Chief of Ordnance, and of a Board of Officers appointed under an Act of Congress, approved June 6th, 1872, a brazed wire-gun of 10-inch calibre had been constructed under his direction at Frankford Arsenal. The fabrication of this particular gun was intended as a means of instruction and practice preliminary to the manufacture of a larger gun; but Congress had added to the work to be accomplished by means of the appropriation asked for by the Chief of Ordnance for this special work, much more largely than to the appropriation, so that it became impracticable to go on with the larger gun. The brazing of so large a mass of wire (about 15 tons) was an operation quite unprecedented, and it might be left to those who had carried out processes so widely varying from anything practised in the arts to say how great were the difficulties of executing the best laid plans in the first endeavour, and whether it was a matter of surprise that the first product should fall short of perfection. It was the gun last mentioned that fell under the Author's condemnation; and it must be admitted that it was by no means a perfect specimen of its style of construction, nor was it ever supposed to be such.

At the ninety-third round, with a charge of 80 lbs. of powder and a 400-lb. projectile, "the gun parted under longitudinal strain," said the record of the Board, 26¾ inches from the bottom of the bore. The powder employed had been selected for its "briskness," with Dr. Woodbridge's approval, and the pressure recorded by the "Woodbridge" gauge, being used alternately with the "Rodman," was 74,400 lbs. per square inch, or, as properly stated by the Board, "about 80,000 lbs. per square inch as measured by the Rodman gauge." Briefly, the defect of the gun was imperfect brazing, consequent upon insufficient heat at the lower or breech part of the gun. Bronze had penetrated everywhere, not a spot being found, in cutting up the gun, which it had not reached. But the lower portion of the gun, except superficially, had been brazed only by "liquated" metal, which alone could flow at the temperature of that part of the mass. Below the plane at

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which the temperature precluded the flow of the bronze in its entirety, the small spaces between the wires were not perfectly filled; and immediately below that plane the interfused bronze held its position, so far as was possible, only by capillary action, until it cooled. It was in this plane of most incomplete brazing that the gun parted. Notwithstanding this result, the abundant heating-capacity of the furnace and the reliability of the tests of the temperature of the different parts of the gun, when properly adjusted and employed, were demonstrated; and no room for doubt was left, at least in his mind, that the original plans, fully carried out, would have assured complete success. One important feature of the construction of this gun seemed to have been overlooked by the Author, who evidently regarded it as a specimen in which the wire had no initial tension. He might have learned from the source of information to which he referred that the gun was cooled from the interior, and that the indications of initial strain, manifested by the compression of the interior, were most clearly given. The bore was contracted, as the result of firing, even at the seat of the charge. Any discussion of the relative advantages of this mode of construction would extend this communication too far. It might be permissible to add that the late Board on Heavy Ordnance who unanimously recommended the construction of two styles of gun presented by him, also recommended, by a majority vote, the fabrication of a brazed wire-gun.

Mr. Longridge.

Mr. **Longridge** in reply to the correspondence said, with reference to Mr. Duval's remarks, that the difference of the dimensions was chiefly in the inner diameter of the powder-chamber, and the effect of this, if introduced into the calculation, would be considerably to increase the tensions of the coil under fire, and the compression of the case at rest, thus showing the gun under worse conditions than actually considered in the Paper. The substitution of the steel strap for the bolts to resist the longitudinal strain, was no doubt a very great improvement. Dr. Woodbridge said that the calculations in the Paper seemed to have been made "without reference to the deformation of the wire, resulting from the imperfect contact of surfaces pressed together by the radial strains generated in winding." In this remark he was mistaken. It was the very contact of one wire with another that transmitted the strains, whether the gun was at rest or under fire, and the effect of this contact, and the normal forms arising from it, had been fully taken into account. The filling the interstices, if there were any, with solder, would not at all increase the strength of the gun. There was nothing in the remarks of Dr. Woodbridge in his

Mr. Longridge. present communication beyond what he had so often st other occasions, and the Author could not see that they way tended to modify the conclusions respecting Dr. Wood gun, which he had embodied in the Paper itself.

25 March, 1884,

EDWARD WOODS, Vice-President,
in the Chair.

The discussion upon the Paper by Mr. Longridge, on gun Construction," occupied the whole evening.

1 April, 1884.

Sir J. W. BAZALGETTE, C.B., President,
in the Chair.

The following Associate Members have been transferred to the class of

Members.

Joseph Bernays.
George Cartwright.
Charles Colson.
William Irlam Ellis.
Alan Grant-Dalton.
James Charles Inglis.
Arthur Shanks.

The following Candidates have been admitted as

Students.

James Hartley Abbott.
Henry Matthew John Bacon.
John James Bourne Benson.
Henry John Bridgewater.
Herbert Alexander Caffin.
Arthur Pitt Chambers Cary.
John Jacob Cohen.
Alfred Fawcus.
John Jervis Garrard.
Albert Daniel Greatorex.
Bertram Jones.
Cyril Edward Arengo Jones.
Hugh Torrance Ker.
James Percy Knight.
Hubert Bindon Marten.
William Pollock.

The following Candidates were balloted for and duly elected as

Members.

Frederick Beesley.
Jorge Rademaker Grünewald.
Clayton Turner Mason.
John Taylor.

Associate Members.

José Barbalho Uchôa Cavalcanti.
John Henry Clemes.
Charles Edward Goodfellow.
Francis William Gosling.
Frank Gotto.
Archibald Greenlees.
Henry Charles Kirstein.
John Edmund Phipps Lincké.
Francis Ronan Mahony.
Charles Leslie Stewart Mais, A.K.C., Stud. Inst. C.E.
Henry Waterworth Parkinson.
George Rankin, Stud. Inst. C.E.
Thomas Harold Rawson.
Martin Fenn Roberts.
Henrique Scheid.
William Acheson Traill.
William Whittington.

Associate.

Tolmie John Tresidder, Capt. R.E.

(*Paper No. 1980.*)

"Experiments on the Composition and Destructive Distillation of Coal."

By William Foster, M.A. (Cantab.), F.C.S., Lecturer on Chemistry at the Middlesex Hospital.

The composition of coal, as determined by the processes of ultimate chemical analysis, has been worked out, more or less, for the best known examples found in the United Kingdom. Very few, however, of such analyses are complete. For instance, most experimenters give the weight of the oxygen and nitrogen collectively and not individually. At the time when many of the published analyses of coal were made, the residuals of coal-gas manufacture, and especially ammonium-salts, had but little value; and it is probably in consequence of this circumstance that the chemists of the time regarded with indifference the determination of the amount of nitrogen in the samples of coal which they submitted to the processes of ultimate analysis. Seeing that the production of ammonia in the destructive distillation of coal is in some degree dependent on the amount of nitrogen present in such coal, it becomes all the more desirable at the present time to have carefully worked out data under this head. Some are given in the Tables attached to this communication.

In addition to the quantitative determination of all the important elementary constituents of each of six samples of coal, a number of observations have been made on the disposition of the elementary constituents of each sample when it is submitted to the process of destructive distillation in a close vessel. With this view complete analyses of the samples of coke produced have been made. This method of inquiry does not appear to have been adopted before. It is not proposed in the present Paper to deal with all the facts brought to light in the course of this inquiry, but to leave the more abstrusive and scientific for consideration elsewhere.

The specimens of coal examined represent all the best known types found in different parts of the United Kingdom, and have been taken from widely separated localities, with the view of making the results as representative as possible. The inquiry having been begun with a particular regard to the character of the gaseous compounds obtained, the samples have been numbered in the order of their gas-producing qualities. Specimen No. 1 is a Scotch cannel of excellent quality. Such a coal does not "cake" in the retort; the pieces of coke obtained retain the shape of the original masses of coal. No. 2 is an English cannel, though probably not a representative sample of English cannels in general. In fact, its chemical composition and behaviour on the application of heat show that it is more nearly related to the "caking" than to the cannel class. In fracture and appearance it is very like the Scotch cannels, but when heated in the retort the pieces fuse together, though not sufficiently thoroughly to destroy the outlines of the original masses of coal. Nos. 3, 4 and 5 are examples of "caking" coals such as are largely used for gas- and coke-making, and are from collieries favourably known in these industries. Though the quality of the illuminating gas which they yield is much inferior to that of the cannel class, on the other hand, they are capable of furnishing the best descriptions of coke. Nos. 3 and 5 are Yorkshire coals, the former being a coal of remarkably good quality. No. 4 is a Durham coal. No. 6 is a good quality of Welsh anthracite, yielding, as is usual with coals of this class, but little gas, which is devoid of illuminating properties.

RESULTS OF ULTIMATE ANALYSES OF COAL AND COKE.

In Table I. are given the particulars of the average results obtained in the case of each coal, and the coke which it produces when heated in a close vessel until all gaseous matter has ceased to come off. Each element present has been determined in the most approved method, and occasionally different methods have been employed, so as to ensure the greatest possible accuracy. The carbon and hydrogen have been determined by combustion in pure oxygen in the presence of chromate of lead; the nitrogen either by combustion with copper oxide *in vacuo*, or with excess of soda lime; the sulphur by fusion with alkaline carbonate and nitrate; the moisture by absorption by oil of vitriol in a sealed atmosphere; the ash by combustion in a muffle furnace. The oxygen has been ascertained in the ordinary way by difference; that is, after having determined the amounts of all the other elements by the direct methods above named and reduced the results to percentages, the deficiency required to make up one hundred parts is taken as the percentage of oxygen. As there is not a direct method for finding the amount of oxygen of an organic substance, the accuracy of the usual estimate is entirely dependent on the working of the methods employed in the estimation of each and all of the other elementary constituents. In the case of the analysis of coal, the possible error in the estimate does not show itself so markedly as in that of the coke, because in the latter there is but little oxygen.

PROXIMATE ANALYSES OF COAL.

In order fully to realize all the points brought out by the complete analysis of a sample of coal and of the coke which it produces, it is necessary to have accurate determinations of the amount of coke obtained from a given quantity of the coal. A series of observations has therefore been made with this view, and has been performed under exactly similar conditions. The particulars of the results are given in Table II. In these experiments 5 grams of coal have been placed in a platinum crucible, provided with a neatly fitting cover, and the whole has been strongly heated for thirty minutes by means of a gas-jet of constant size. In about ten minutes the whole of the gas from the coal appears to have escaped, so that it is certain the coke is of a representative description if the heating be maintained for half an hour. The crucible having been quickly cooled has then been weighed, and the percentage of dry coke and volatile matter deduced. Such percentage determinations are absolutely needed for a critical and comprehensive consideration of the disposition of the elements composing the coal. This Paper more particularly deals with the sulphur and nitrogen.

The samples of coke obtained in this way have been employed for the purposes of ultimate analysis. Before being employed in these operations they have been reduced to the finest powder possible, and have been carefully preserved in well-stoppered bottles. In spite of all ordinary precautions the samples of coke have occasionally taken up a tolerably large amount of moisture from the air, and in that respect have shown a more marked tendency to absorb moisture than coal when in a similar condition. The determination of the amount of moisture, whether in the case of coal or of coke, has led to some special observations, the outcome of which is, that it is impossible to rely on the numbers obtained by the usual method of drying the substances at an exalted temperature in a drying oven. Whether it is owing to the action of the components of the coal or coke as a whole, or whether it is entirely due to the sulphur only, no opinion is offered. The fact still remains that oxidation (which means increase in weight) does occur at an exalted temperature simultaneously with the removal of moisture, which tends to diminish the weight. The former may be entirely prevented, and the moisture correctly ascertained, by allowing a weighed quantity of the material to stand over a surface of oil of vitriol for twenty-four hours in a sealed atmosphere of limited dimensions. This course has been adopted in all the experiments given in Table I. except in that of the Durham coal, No. 4. This particular analysis was made some time ago, and was published last year. The coke which it produces is now added.

DISPOSITION OF THE SULPHUR OF COAL BY DESTRUCTIVE DISTILLATION IN A CLOSE VESSEL.

The actual condition of things revealed by these experiments is unexpected, and in opposition to the ideas prevailing on the subject. For instance it is generally accepted as an axiom that coke contains more sulphur than coal. The strongest popular indictment levelled against the use of gas-coke as a domestic fuel is based on this belief. It is therefore a significant fact that, throughout this series of samples of coal and coke, in no case is the percentage of sulphur in the coke so great as in that of the parent coal; and in several cases it is considerably less. The behaviour of the sulphur of coal may be considered from two standpoints: one that of the gas engineer, the other that of the ironmaster. The former deals particularly with the sulphur, in its various combinations, found in the crude coal-gas, and has no special interest in knowing what amount of sulphur remains in the coke, or its fate. The reverse of these conditions interests the ironmaster, inasmuch as he endeavours to obtain coke as free from sulphur as possible, and with that view not only selects the coal, but goes to the further trouble of picking and washing by machinery, at least in special cases.

The various analytical numbers relating to sulphur given in Table I. have been simplified and arranged in Table III. The behaviour of the sulphur of a particular sample of coal is stated in two ways in two adjacent columns. There is in the first column the amount of sulphur relating to 100 parts of coal which is expelled and found in the crude coal-gas, and beneath these figures are those which denote the amount of sulphur remaining behind in the coke from 100 parts of coal. In the adjacent column the same facts are expressed as percentages on 100 parts of sulphur present in the original coal. For the purposes of this Paper it will no doubt be conceded that the sulphur, whether of coal-gas or of coke, is a source of mischief, or of expense, or of both. The sulphur found in the coal-gas is more interesting than that remaining in the coke, because the former can be and is removed, although to an extent which varies considerably, according to the legislative enactments relating to the gas supplied by a particular company or corporation.

The sulphur in crude coal-gas may be regarded as being present in two forms: the one sulphuretted hydrogen and the other bisulphide of carbon, together with nondescript sulphur compounds. It is usual to speak of the latter, including the bisulphide of carbon, as the "carbon-sulphur compounds." Where the purification of the coal-gas is strictly limited to the removal of the sulphur existing as sulphuretted hydrogen, this is effected by oxide of iron which entails the minimum expense. Probably the recent attempts of two or three gas engineers to use up the sulphur removed in this way, on the spot, may be attended with profit. No data are, however, available at present. In most cases the coal-gas is specially treated for the removal of the carbon-sulphur compounds, the processes as a whole, not being of the simple and inexpensive nature which especially characterises that for the removal of sulphuretted hydrogen. Therefore, considering the question of sulphur-purification in its broadest sense, and as practised by the London and other gas-companies, it must be regarded as a matter which entails a loss dependent, within certain limits, on the amount of sulphur in all forms present in the crude coal-gas. In order still further to simplify the results expressed in Table III., it will be assumed that 1 ton of each of the first five kinds of coal under notice yields 10,000 cubic feet of gas. The figures for the sulphur in the crude coal-gas expressed in lbs. per 1,000 cubic feet are as follow:—

TABLE SHOWING the AMOUNT of SULPHUR EXPRESSED in lbs. per 1,000 CUBIC FEET of CRUDE COAL-GAS from FIVE DIFFERENT SAMPLES of COAL.

Coal No. 1.	Coal No. 2.	Coal No. 3.	Coal No. 4.	Coal No. 5.
4·233	1·075	0·716	0·515	2·844

The maximum amount of sulphur allowed in the gas supplied by the London companies at the present time (winter months) is 220 grains or 0·031 lb. per 1,000 cubic feet. The removal of such varying amounts of sulphur as are specified above, so as to reduce the quantity in the manufactured product within the limits defined by law, must be attended with an expense related in some degree to the amounts of sulphur in the crude gas. The most noteworthy feature in connection with this part of the subject is that the coal marked No. 3, which contains such an exceptionally small percentage of sulphur, furnishes a gas containing one and a half time more sulphur than coal No. 4, although the percentage of sulphur in the latter is double that of the former. Further, No. 3 yields more volatile matter by 5½ per cent. than No. 4, and this extra yield may be connected in some way with this increased elimination of sulphur from the coal. Such a view is strengthened by a consideration of the behaviour of coals No. 1 and No. 2, No. 1 in particular furnishing a large amount of volatile matter correspondingly rich in sulphur.

DISPOSITION OF THE NITROGEN OF COAL DURING DESTRUCTIVE DISTILLATION IN A CLOSE VESSEL.

This phase of the work connected with the present researches has been the subject of much experiment extending over the past two years. In the course of a discussion at the annual meeting of the Gas Institute in 1882, an account was given of the behaviour of the nitrogen of Durham coal;[1] and subsequently the same subject was considered in a short Paper by the Author, read before the Chemical Society.[2] The results referred to in these communications are incorporated in the present Paper and appear in Table IV. under the head of coal No. 4 (Durham). The method adopted in the distillation of the Durham coal was slightly different from that practised in the case of the other samples, namely Nos. 1, 2, 3 and 5. The later experiments have been made by distilling weighed quantities of about 1½ oz. of coal in a cylindrical iron retort, whereas the experiments on the Durham coal were made in glass combustion tubes.

When coal is submitted to destructive distillation in a close vessel, the disposition of its nitrogen may be considered under four heads. According to the experiments about to be described from 11 to 18 per cent. of the total nitrogen takes the form of ammonia-gas or its compounds; from 0·2 to 1·5 per cent. being found as cyanogen gas; from 21 to 36 per cent. is unaccounted for, and is believed to be in the crude coal-gas as free nitrogen; and from 48 to 66 per cent. remains behind in the coke. Table IV. gives the figures expressing the disposition of the nitrogen of samples 1 to 5 inclusive arranged under the four heads already named. Double columns are used for each sample of coal. In the first column is the amount of nitrogen in each of the four conditions expressed as a percentage on 100 parts of the original coal, and in the second column the same facts are expressed as percentages on 100 parts of nitrogen. For further simplicity and comparison the ammonia obtained from each specimen has been expressed as lbs. of ammonium sulphate per ton of coal.

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[1] The Gas Institute. Transactions for 1882, p. 188.

[2] "The Behaviour of the Nitrogen of Coal during destructive distillation: with some observations on the estimation of nitrogen in coal and coke." Journal of the Chemical Society, No. ccxliii., February 1883, p. 105.

AMMONIA IN THE CRUDE COAL-GAS.

The method of obtaining the figures under this head has been to place 40 grams of the coal in a cylindrical 1-inch iron tube about 22 inches long, and to raise the whole to the temperature of distillation as quickly as possible. Each observation has been carried out on the same lines as far as has been practicable. The ammonia of the crude coal-gas has been removed by dilute acid, and this in turn has been distilled with excess of caustic soda, the ammonia having been eventually weighed as platinum ammonium chloride. It is a matter for regret that the most important question of temperature has not received, during the course of these inquiries, that scientific treatment which in the present state of knowledge is so much needed. What has been aimed at is, that the results obtained with the different samples of coal should, as far as possible, be strictly comparable; and the object has been not so much to ascertain what is the greatest amount of ammonia which can be obtained, as to ascertain what is obtained in the ordinary method of conducting the destructive distillation of coal, unaided by modifying conditions. It is believed that the results obtained with coals numbered 1, 2, 3 and 5 are comparable. No. 4 was distilled in a glass tube. Viewing the figures as a whole, the small proportion of nitrogen which is evolved as ammonia is noticeable; this, in the highest case, is 17·8, and in the lowest 11·1 per cent. Curiously enough, the specimen of coal which contains the lowest percentage of nitrogen (1·28 per cent.) yields as much ammonia as any of the other examples, although some of them are much richer in nitrogen. Average results are given in all cases except No. 4. Had the maximum result only been taken, coal No. 1 would have shown almost as high a yield of ammonia as that accorded to No. 4. Coal No. 1 certainly yields far more volatile matter than any other of the coals experimented with; and therefore the greater yield of ammonia may be related in some way to this circumstance. Clearly this of itself does not appear to be the full explanation, because other coals (and notably No. 4) are nearly as good ammonia producers, though furnishing far less volatile matter. The explanation is undoubtedly to be found in some fundamental difference of nitrogen itself, which, although a gas at ordinary temperatures, is, when in chemical combination with carbon as a solid compound, next to carbon in its power of resisting tendencies to chemical change brought about by mere elevation of temperature. The negative character of nitrogen has always been considered a property favourable to its ready elimination in some gaseous form or other when coal is submitted to destructive distillation, and this idea seems to have been entertained by all writers on the subject.

CYANOGEN PRESENT IN THE CRUDE COAL-GAS.

After the crude coal-gas has been treated with dilute acid so as to remove all ammonia, it can still be made to furnish a much smaller quantity by being passed through highly-heated slaked lime. The method of operating has been to remove all ammonia from the crude gas, and then to pass the latter through a tube about 2 feet long filled with slaked lime, and maintained at a high temperature in a furnace. The gas escaping at the outlet has again been washed with dilute acid, and from the acid liquor the ammonia has been distilled and weighed as platinum ammonium chloride. The substance which is the origin of this second quantity of ammonia is cyanogen or its compounds. The amount is small, and appears to be increased by the employment of high temperatures during the distillation of the coal. The highest yield in the experiments recorded in Table IV. was furnished by the Durham coal, which was distilled in a glass tube; and there is no doubt that a higher average temperature was maintained during this experiment than in the other cases where an iron retort was used. This opinion is based not only on the amount of cyanogen produced, but also on the relatively large amount of nitrogen which was unaccounted for owing to its being in the crude gas in the elementary condition.

NITROGEN UNACCOUNTED FOR.

The greater portion of the nitrogen classed under this head is believed to be present in the crude coal-gas in the elementary condition. Some nitrogen undoubtedly exists in the tar in the form of alkaloidal substances which are not gaseous at ordinary temperatures. The quantity thus found in the tar is assumed to be relatively small. No data are available for computing the actual amount. The cause of the existence of so much free nitrogen in the crude coal-gas is one which is of the greatest importance to the gas-engineer. The Author believes that it is increased within certain limits by increase of temperature, other things being equal. This part of the subject, however, is somewhat obscure. In 1882 a large number of observations was made by the Author on the amounts of ammonia in crude coal-gas at different stages during the process of distillation,[1] and the results then obtained distinctly showed that, in the process as carried on in the works where the experiments were conducted, the coal-gas was particularly poor in ammonia-gas at the latter period of the operation. The Author attributes the existence of free nitrogen in the crude coal-gas not so much to the evolution of the element as such from the coal, but rather to the resolution by the action of heat of the ammonia first evolved from the coal into its two elementary constituents. This opinion is arrived at not only from a consideration of the experiments above cited, but also from the results given in the present Paper in so far as they relate to this point. A careful inquiry as to the influence of temperature in modifying the yield of ammonia (and indirectly of other substances) is much needed. In suggesting this to the attention of the gas-world the following points may be noted: 1, the use of a coal of carefully ascertained composition; 2, the careful determination of the amount of ammonia coming off at different periods of the distillation, and its total; 3, the composition of the coke resulting from each operation; 4, the employment of different temperatures, which should be ascertained, and an average arrived at for each operation of distillation.

NITROGEN REMAINING BEHIND IN THE COKE.

The amount of nitrogen which remains behind in the coke in the case of the five examples in Table IV. ranges from 49 to 66 per cent. of the total nitrogen present in the parent coal. The

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[1] The Journal of Gas Lighting, vol. xl., p. 1125.

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amount found in the sample of Durham coke, No. 4, has already been published. The most notable features in connection with the remarkable results furnished under this head are those obtained with coal No. 1. This coal contains a considerably lower percentage of nitrogen than any of the other coals in the list; yet, as has been seen, it is almost as good a producer of ammonia as the best of the series. The coke which is produced is also considerably richer in nitrogen than the parent coal. This apparently anomalous result can only be understood on some such hypothesis as has already been put forward; and exceptional as the behaviour of the nitrogen of this particular coal may seem at first, yet when the results of the distribution of the nitrogen under the four heads are considered as percentages on 100 parts of original nitrogen, the anomaly in a great measure disappears.

Having demonstrated that coke from the most varied sources contains so much nitrogen, the next step has been to ascertain its condition and behaviour under circumstances favourable to its elimination in the form of the valuable product ammonia. Much has been written and said on this subject at different times since the publication of the Author's earlier experiments a year and a half ago. It is not possible in this Paper to deal with all the arguments and methods adopted by the many observers whose attention has been attracted to the subject, although it is desirable to notice some. In the first place, it is important to observe that the nitrogen is in a very stable form of chemical combination with the other elements of the coke, and most probably more particularly with the carbon. The nitrogen certainly does not exist in the coke as ammonia, although this has been freely stated to be the case. If finely-powdered coke be treated with any of the ordinary chemical reagents sufficiently energetic to evolve ammonia from a mixture which contains it already existing as such, no indications of the gas in question can be obtained. On the other hand, the usual methods for the determination of the nitrogen of an organic substance apply, with some modification, to the estimation of the nitrogen of coke. All of it can be obtained as ammonia by heating the very finely-powdered coke to dull redness with an intimate admixture of soda lime in excess. But in order that the operation may be completely successful, it is necessary to maintain the application of heat for a longer time than would suffice for the similar treatment of a sample of coal or any ordinary "organic" substance. The chemical stability of the coke is very pronounced, not only in the case of the use of soda lime, but in other operations of a kindred nature.

When coke is maintained at a red heat in a current of steam, the escaping gases, mainly hydrogen and carbonic oxide, contain relatively small quantities of ammonia and sulphuretted hydrogen. As a commercial process this will be considered subsequently. Many experiments have been made on samples of coke obtained from all the first five coals named in the Tables with this general result, that the nitrogen of each is amenable to this treatment with steam. This fact being established, and knowing the large amount of reserve nitrogen in the coke, the possibility of producing considerable quantities of ammonia is a natural sequence. It is, however, unnecessary to relate the many results which have been obtained, since they have been anticipated by the recently published experiments of others.

The action of steam in producing ammonia from coke can be made to furnish some very important evidence which, although somewhat disappointing in the prospects that it foreshadows, defines most clearly the maximum advantage that can be obtained by its use, and also throws some light on the condition of the nitrogen existing in the coke. A quantity of coke was prepared from coal No. 1 (Scotch cannel). It was believed that the richness in nitrogen, and the porous character, of this particular coke made it specially suitable for the following inquiry. A weight of 50 grams perfectly dry was placed in the iron cylindrical retort, the whole was raised to a red heat in the furnace and maintained in that condition, steam largely in excess being then passed through the apparatus for definite and consecutive periods. The gases produced were washed with acid in the ordinary way, and the amount of ammonia absorbed during each period was eventually estimated. At the end of these operations the coke was removed and weighed, though not with the strictest accuracy. Its weight was recorded as about 43 grams, that is, the coke had lost about 14 per cent. in weight by being subjected to the action of steam at a red heat for four and a half hours. The amount of nitrogen in the coke from coal No. 1 (Scotch cannel) is given as 1·47 per cent. in Table I.; consequently, on the assumption that the specimen in question contained this amount of nitrogen, the 50 grams of coke employed would contain 0·735 gram. As 0·1148 gram was obtained in the form of ammonia, this quantity can be expressed as a percentage on the presumed total original nitrogen. It is equal to 15·6, and is almost identical with the maximum quantity possible, unless the action of steam is attended with some special selective influence on the nitrogen of the coke, causing it eventually to be expelled from the retort as free nitrogen.

The result of the foregoing experiment expressed as ammonium sulphate is equal to 24·9 lbs. per ton of coke treated, and that with only a loss of weight of 14 per cent. On the assumption already made, a further loss of 14 per cent. in weight under the influence of steam ought to be attended with another corresponding yield of ammonia. Important as the foregoing experiments are in the lessons they teach, there still remain many points on which it is desirable to have accurate data.

The experiments were therefore repeated, having special regard to the following heads:—1, the quantity of nitrogen in the original coke; 2, the amounts of ammonia produced during different stages of the operation of steaming; 3, the quantity of steam used; 4, the loss in weight of coke by the action of the steam; 5, the quantity of nitrogen remaining in the steamed coke.

The coke used was made in quantities of a few ozs. at a time from the Scotch cannel coal, and the different batches were eventually well mixed. On subjecting some of the mixed specimen to the action of steam, the production of appreciable quantities of tar showed that the coking of the coal had not been complete. One determination of the nitrogen in the well-mixed sample by the soda lime process gave 1·38 as the percentage. This is lower than would have been the case had the coking been pushed further. There were now placed in the experimental retort 50 grams of the coke, the whole being heated to redness and maintained in that condition, and steam passed through in large excess.

The following are the particulars of the two experiments:—

ACTION OF STEAM IN PRODUCING AMMONIA FROM COKE.

Period during which Steam was passing through the Apparatus.		Amount of Nitrogen found as Ammonia.	
First Experiment.	Second Experiment.	First Experiment.	Second Experiment.
Minutes.	Minutes.	Gram.	Gram.
45	45	0·0112	0·1400
90	90	0·0196	0·1204
135	90	0·0840	0·0896
..	90	..	0·0756
Totals 270	315	0·1148	0·4256

The coke on being removed from the retort, at the termination of the second experiment, weighed 29 grams, showing a loss of

42 per cent. on the original. The steam passed into the retort during the whole operation was 500 grams. One determination of the nitrogen remaining in the steamed coke was made by the soda-lime process, and gave 0·831 per cent. From these data important conclusions can be arrived at. The nitrogen present in the 50 grams of original coke is 0·69 gram. The nitrogen actually recovered as ammonia by the operation of steaming is 0·4256 gram, and the difference between these two quantities is 0·2644 gram, and represents the theoretical quantity of nitrogen remaining in the steamed coke. The actual quantity present in the 29 grams as shown by the nitrogen analysis is 0·241 gram. There is therefore some loss of the nitrogen of coke regarded as ammonia, even when coke is heated in a large excess of steam, but it is extremely slight. Calling the quantity of nitrogen in the original coke 100, the following is the disposition of the nitrogen by this operation of steaming:—

Nitrogen obtained as ammonia	61·6
Nitrogen unaccounted for	3·4
Nitrogen remaining in the steamed coke . .	35·0
	100·0

The nitrogen unaccounted for undoubtedly arises from a resolution of some of the ammonia first formed into nitrogen and hydrogen by contact with the heated surface of the iron retort; and the fact that the quantity is small in this instance appears to throw some light on the origin of the unaccounted for nitrogen in the case of the simple distillation of raw coal.

The acquisition of 61·6 per cent. of the total nitrogen of a sample of coke by a loss in weight of the coke of only 42 per cent. demonstrates that steam at a high temperature does exercise some specially selective influence on the nitrogen of the coke, although not to such an extent as could be wished. The foregoing quantity is equal to 90 lbs. of ammonium sulphate per ton of coke treated; and it is obvious from the analysis that the steamed coke is still capable of yielding about 50 lbs. more sulphate on the original ton of coke; in other words, that it is perfectly easy to obtain 140 lbs. of ammonium sulphate from 1 ton of coke containing the particular quantity of nitrogen as set forth in the analysis.

The foregoing data afford a means of comparison between the steam actually used and that theoretically necessary. Assuming that the coke employed is pure carbon, the 21 grams loss of weight sustained by the 50 grams of original coke would require 31·5 grams of water as steam for the equation

$$C + H_2O = CO + H_2.$$

But 500 grams of steam were passed into the retort. Hence in this particular experiment no less than sixteen times the theoretical quantity of steam were employed, while the coke in the retort was maintained at a bright red heat by the application of heat externally. It may therefore fairly be inferred that the conditions of the experiment were in the highest degree favourable to the production of ammonia, and also to the "gasification" of the coke.

The limited dimensions of the apparatus employed have prevented the carrying out of trustworthy experiments in which steam could be used in quantities just sufficient for the purpose of gasification. The Author is aware of the mechanical difficulties which would have to be overcome in order to carry out the process on a commercial scale without undue waste of steam; but he believes that they are not insuperable. It has therefore been assumed, in the course of subsequent considerations, that steam could be employed, not unduly in excess, and that ammonia would be obtained by such treatment in the proportions demonstrated by these experiments.

SOME TECHNICAL POINTS ARISING FROM A CONSIDERATION OF THE FOREGOING ANALYSES AND EXPERIMENTS.

At the present time much attention is being bestowed on the subject of gaseous fuel, and its extension is being promoted. There can be no question that its possibilities in a large number of industries are as yet but imperfectly realized, and a great extension in this direction may reasonably be expected. Earnest attempts are also being made to extend the use of gaseous fuel for domestic purposes. Its cleanliness, the absence of smoke in its use, and the economy attendant on its adoption as an intermittent source of heat, are its chief recommendations. Its promoters may be arranged in two classes: those who recommend the supply of a "producer," or "water-gas" by separate mains to private consumers, and those who would employ coal-gas for both heating and illuminating purposes, making it possible to do so by reducing the cost of the coal-gas to the consumer. The promoters of the extension of coal-gas for this twofold purpose have undeniably a strong case, from the fact that the gas companies are already "in possession" with the plant and appliances in working condition.

By extending the "heating" uses of coal-gas it is further maintained that the consumption of the gas will be more uniform during a greater portion of the twenty-four hours than it now is, and that therefore the capital charges for plant in the works will not be proportional to the increased consumption, if brought about as suggested. One result of the extension of the use of coal-gas is not usually contemplated, namely, the increased production of coke. At present there is some difficulty in London in disposing of gas-coke at a price commensurate with its heating value; and some cities and towns in the United Kingdom are worse circumstanced in this particular than London. It therefore behoves the promoters of the extension of coal-gas for use in domestic fires to take into consideration the contingent increase in the production of coke, and to provide for it; because a depreciation in the market price of the coke would operate unfavourably in the direction of coal-gas being supplied at a lower price. A combination domestic fire, in which gas-coke could be employed with a limited amount of coal-gas to render the whole cheerful, would be a step in the right direction, and would meet the wants of those requiring a fire for short periods at short notice, say in bedrooms and the like. It would also satisfy the requirements of the promoters of smokeless fuel. Sufficient has already been said, under the head of "Sulphur in Coke," to show that coke is a fuel of the best description. It however requires some care and intelligence in its use.

The Author has more particularly concerned himself with the question of applying coke to the production of "water-gas"; and in the first instance was led to hope that the large amount of reserve nitrogen of the coke could in some way be converted into ammonia in the process. That coke can be treated so as to furnish its nitrogen as ammonia has been demonstrated at some length in the foregoing pages; but whether such a process can be conducted on a commercial scale for the supply of "water-gas" for domestic purposes, bearing in mind the present position of the coal-gas industry, is not by any means so easy to demonstrate. Whatever may have been the prospects of the "water-gas" industry (and they have never been very hopeful in this country), it is certain that they were never brighter than at present, seeing that ammonia can be simultaneously produced as a by-product. It has also been found during the course of these experimental inquiries, that the sulphur of coke is evolved as sulphuretted hydrogen when the coke is subjected to the action of steam at a red heat; and as sulphur in this form of chemical combination is readily dealt with and can be turned to profit, especially when produced simultaneously with ammonia, there is every prospect of the sulphur of coke occupying the position of a second by-product of some little value.

It is important to point out that the possibility of the easy removal of the sulphuretted hydrogen from "water-gas" is a great feature in the scheme for supplying gaseous fuel free from sulphur, for use in a number of industries where the presence of sulphur in the fuel is most objectionable. The Author is convinced that some of the Sheffield and Birmingham industries would greatly profit by the substitution of gaseous fuel free from sulphur for the gas-coke now used, which, though washed and treated at great length, retains its original sulphur, and he believes that the advantages of sulphur-free, smokeless, gaseous fuel will before long be practically realized in these and similar districts.

The chief point aimed at in this portion of the present work has been to ascertain the advantage likely to arise from the recovery of ammonia as a by-product. Given 100 parts of gas-coke, it can be approximately predicted from its percentage amount of nitrogen what will be the amount of ammonia recoverable in the gasification of the coke by the agency of steam. But in the process of gasification fuel is necessary; and therefore the simplest and most accurate method of calculating the possibilities of ammonia-production, and the manufacture of "water-gas," is to consider what portion of the 100 parts of coke will be needed to gasify the remainder, taking as data the usually accepted thermal equivalents of the books and the results of the present inquiry. The condition of things expressed by the chemical equation—

$$C + H_2O = CO + H_2,$$

where carbon and steam mutually reacting give rise to carbonic oxide and hydrogen in equal volumes, is that which every producer of gaseous fuel keeps before him as his ideal.

Having steam and carbon at the temperature required for the above decomposition to occur, the 18 parts of steam expressed by the formula H_2O require 68,068 heat-units to be imparted to them before they are completely separated into gaseous hydrogen and oxygen. The combustion of the 12 parts of carbon expressed by the symbol C in the oxygen freed from chemical combination with hydrogen is attended with the production of 29,676 heat-units. The difference between these two quantities is 38,392, and expresses the net amount of heat which must be imparted to bring about the necessary chemical changes. There is still remaining to be considered the amount of heat required to raise the 12 parts of carbon and 18 parts of water from an ordinary temperature to that of mutual chemical decomposition. If this be regarded as 600° Centigrade, the specific heat of carbon as 0·24, the specific heat of steam as 0·48, and the initial temperature as 10° Centigrade, the heat-units will be as follows:—

Carbon	12 × 0·24 × 590	= 1,699
Water to steam	18 × 626	= 11,268
Steam to 600°	18 × 0·48 × 500	= 4,320
Heat-units to effect decomposition at a temperature of 600° C.		= 38,392
Total		55,679

The combustion of 12 parts of carbon in oxygen to carbonic acid is attended with the production of 96,960 heat-units, and therefore from the foregoing estimate the necessary heat-units (55,679) would be provided by the combustion of 7 parts of carbon. The amount of heat expended in conducting the operation on a commercial scale would, no doubt, be much in excess of the theoretical quantity. Therefore it is proposed to allow 100 per cent. more fuel for the gasification of coke than is theoretically needed; that is to say, every 12 parts of carbon shall be supposed to require 14 parts of carbon for their gasification. A study of the analysis of coke shows that dry coke contains about 90 per cent. of carbon; and, supposing 42 per cent. of a sample to be gasified, there would remain 48 parts of carbon available for the purposes of fuel for conducting the process. The ratio of 48 to 42 is almost identically that of 14 to 12. It has already been shown that the gasification of 42 per cent. of a sample of coke is attended with the production of 90 lbs. of ammonium sulphate per ton of coke treated; and therefore, if 100 tons of coke were subjected to this limited influence of steam, 4 tons of the ammonium salt could be obtained. Taking the net value of the salt at £10 per ton, there is at once £40 to the credit of this residual.

A simple way of considering the heating value of the gaseous fuel is to compare it with that of the coke employed both in and for the purpose of gasification. An allowance of 14 parts of carbon has been made as fuel for the conversion of 12 parts of carbon into "water-gas," and therefore the computed heat-units must be on a basis of 26 parts of carbon. These 26 parts, if burnt to carbonic acid in the ordinary way, would develop 210,080 heat-units. The total heat-units developed by the combustion of the hydrogen and the carbonic oxide, the results of the gasification of 12 parts of carbon, are 202,636. Therefore, on the above basis, the theoretical value of the "water-gas" as a heating agent is only about 4 per cent.

less than the theoretical heating value of the whole of the coke used in its manufacture, were that coke consumed direct to the condition of carbonic acid.

The greater efficiency of gaseous fuel enables it to be substituted for solid fuel in a large number of operations, and the fact that a larger percentage of the heat developed can be turned into useful effect in many special cases is the cause of its wide-spread adoption. Any process, therefore, which tends to reduce its cost of production, whether directly or indirectly, can only tend to augment the advantages it is known to possess when employed in certain industrial operations.

The natural field for the manufacture of "water-gas" is an ordinary gasworks. Coke is often a residual of little value in the district, and occasionally, from a number of circumstances, it has to be sold at a remarkably low price. The more advanced large works are gradually, as circumstances permit, adopting gaseous fuel for the purpose of heating the retorts. There is, therefore, every opportunity of introducing the manufacture of the best description of "water-gas," if only for consumption on the premises. In this way the ammonia obtainable could be saved, and the whole process submitted to a thoroughly practical test. If successful, the manufacture could be extended so as to supply external demands as they arose. The advance would be a natural and an easy one. At present the gasworks supply the great bulk of the material used in artificial illumination. They are also large producers of solid fuel in the shape of coke. Further, they are the producers of the material for the manufacture of ammonium salts for the world generally. The propositions now made are therefore not calculated to disturb the existing order of things, and it is believed that their adoption would prove a step in the right direction, and in accordance with the spirit of the times. The utilization of the nitrogen of coke would enable a gasworks to keep up its revenue from the sale of ammonia owing to the reduction in the price of ammonium salts, and to maintain a fair position in the face of the existing competition with the owners of collieries. The agriculture of this and continental countries needs all the ammonium salts likely to be produced.

It is twenty years since the late Sir William Siemens introduced gaseous fuel for the heating of retorts in the Paris gasworks, with perfect success. Gaseous fuel has not been employed for the same purpose in this country for more than three or four years. The most advanced engineers now admit its superiority to solid fuel, and are adapting their appliances for its employment as opportunities offer. In fact, they have adopted it after the lengthened experience of other industries—a strictly safe course. Will a similar series of events attend the progress of the "water-gas" industry?

It is not possible here to do more than barely notice the labours and writings of several gentlemen who have been engaged on some of the subjects discussed in this Paper. Since the publication of the Author's discovery of the relatively large amount of the nitrogen of coal which remains behind in the coke, earnest attempts have been made to win it for useful purposes. The first to do so in point of time was Mr. William Young; and he has been followed by Mr. Scheurer Kestner, Mr. Beilby, Mr. Tervet, and several others, all of whom have contributed to a knowledge of what is, confessedly, a difficult subject.

APPENDIX.

TABLE I.—RESULTS of ULTIMATE ANALYSES of SAMPLES of COAL and COKE.

Elementary Components.	No. 1. Scotch Cannel.		No. 2. English Cannel.		No. 3. Yorkshire.		No. 4. Durham.		No. 5. Yorkshire.		No. 6. Welsh Anthracite.	
	Coal.	Coke.	Coal.	Coke.	Coal.	Coke.	Coal.	Coke.	Coal.	Coke.	Coal.	Coke.
Carbon	75·42	86·75	81·46	87·64	84·10	88·45	84·34	87·32	82·18	84·04	89·17	88·64
Hydrogen	6·18	1·37	5·02	1·31	4·93	1·41	5·30	1·67	5·31	1·51	3·50	1·37
Nitrogen	1·28	1·47	1·66	1·36	1·67	1·60	1·73	1·13	1·75	1·59	0·91	0·99
Sulphur	2·18	0·65	0·95	0·74	0·55	0·35	0·78	0·75	2·00	1·06	0·96	0·63
Oxygen (by difference)	8·70	0·48	5·00	0·48	5·35	1·67	4·29	1·07	3·66	0·68	1·56	0·60
Moisture	4·00	4·53	2·07	2·67	2·20	4·82	1·14	4·36	1·34	4·08	1·78	5·60
Ash	2·24	4·75	3·84	5·80	1·20	1·70	2·42	3·70	3·76	7·04	2·12	2·17
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00

TABLE II.—PROXIMATE ANALYSES of COAL.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Volatile matter	57·02	37·02	34·44	29·00	33·66	7·88
Coke (dry)	42·98	62·98	65·56	71·00	66·34	92·12
	100·00	100·00	100·00	100·00	100·00	100·00

TABLE III.—DISPOSITION of the SULPHUR of COAL by DESTRUCTIVE DISTILLATION in a CLOSE VESSEL.

	No. 1.		No. 2.		No. 3.		No. 4.		No. 5.		No. 6.	
Sulphur present in the crude coal-gas	1·89	86·7	0·48	50·5	0·32	58·1	0·23	29·5	1·27	63·5	0·30	31·3
Sulphur remaining in the coke	0·29	13·3	0·47	49·5	0·23	41·9	0·55	70·5	0·73	36·5	0·66	68·7
	2·18	100·0	0·95	100·0	0·55	100·0	0·78	100·0	2·00	100·0	0·96	100·0

TABLE IV.—DISPOSITION of the NITROGEN of COAL by DESTRUCTIVE DISTILLATION in a CLOSE VESSEL.

Description of Sample.	No. 1. Scotch Cannel.		No. 2. English Cannel.		No. 3. Yorkshire.		No. 4. Durham.		No. 5. Yorkshire.	
Nitrogen evolved as ammonia (average results, except No. 4)	0·227	17·8	0·192	11·6	0·209	12·5	0·251	14·5	0·195	11·1
Nitrogen evolved as cyanogen	0·005	0·4	Unknown		0·006	0·4	0·027	1·5	0·004	0·2
Nitrogen unaccounted for. The greater portion is believed to be in the gas as free nitrogen	0·387	30·2	0·593	35·7	0·354	21·2	0·610	35·3	0·452	25·9
Nitrogen remaining in the coke in chemical combination	0·661	51·6	0·875	52·7	1·101	65·9	0·842	48·7	1·099	62·8
	1·280	100·0	1·660	100·0	1·670	100·0	1·730	100·0	1·750	100·0
Lbs. of ammonium sulphate from 1 ton of coal	23·96		20·27		22·06		26·40		20·50	

[DISCUSSION.

Discussion.

Mr. Foster.

Mr. W. FOSTER called attention to the Tables, which, he said, presented the averages of four or five experiments in the case of each of some of the figures, the work having gone on for the past two and a half years. Since the Paper had been in the hands of the Institution a great deal of further work had been done; as even after such elaborate Tables the whole subject had not been thrashed out. A large number of workers were still busy on it. With regard to the nitrogen unaccounted for, he had at present no information which would lead him seriously to modify any opinion he had expressed. Long as the Table was, he did not wish it to be regarded as representing all that it might be hoped to ascertain with reference to the nitrogen of coal, and how it behaved in the process of destructive distillation. It would be seen that Table IV. showed lbs. of ammonium sulphate per ton. These figures should be taken with a certain amount of judgment. He had not given the highest or the lowest results, but had taken the average of about half a dozen distillations. If he had taken the highest result, as in the case of Durham coal, possibly the figures for the Scotch cannel would be about the same. The English cannel was what was known as a "bastard" cannel, and not a true cannel; it was similar in composition to ordinary Durham or Yorkshire coal, and gave upwards of 30 lbs. of sulphate by certain treatment in simple destructive distillation. All the figures had been worked out from certain experiments essentially commencing with the retort cold, so that at first the process had been one of distillation at a moderate temperature. The small experimental retort would give the members an opportunity of seeing the condition under which the experiments had been made. They were fairly comparable with the result obtained in ordinary gasworks. Some engineers had obtained 24 lbs. of sulphate per ton; and he had heard it stated by engineers in the north that they had got 32 lbs.; but that statement had been very much questioned. He did not find anything extraordinary in it, seeing that even in the most favourable instances they were only getting 17 per cent. (in no case did it amount to 20 per cent.) of the total nitrogen. With reference to sulphur the figures were very instructive, but they included the sulphur present in crude coal-gas and tar. That again was a problem still in hand.

Mr. Lowthian Bell.

Mr. I. LOWTHIAN BELL said he had read the Paper with great pleasure and interest. It was replete not only with great philosophical truths, but with much that was highly interesting and valuable in a national point of view. He had understood the Author to say that he had nothing to retract from the expectations he had formed with regard to the future. If that were the Author's meaning he was certainly more sanguine than Mr. Bell as to the quantity of ammonia likely to be obtained from coal, however distilled. The expectation was held out that 140 lbs. of ammonia might be obtained from a ton of coke, or, roughly speaking, 100 lbs. for every ton of coal distilled. Now distillers were not altogether without experience in the matter. He had no doubt that the Author was acquainted with the plan now adopted at the Gartsherrie Works, and elsewhere in Scotland, for the collection of sulphate of ammonia from blast furnaces using raw coal. The question was one of great interest to him, and he had carefully examined the works, since which time many analyses had been made of the products. In a Paper by Mr. Weldon, it was mentioned that 20 lbs. of sulphate of ammonia were obtained from each ton of coal treated in the blast furnace. In order that the members might have some idea of its importance he might mention that the value of the sulphate of ammonia, together with the value of the tar, was such that the ironmasters of Scotland hoped, as far as the mere question of cost was concerned, to make their ton of iron almost without any coal at all; in other words, that the value of the tar and ammonia would be nearly equal to the cost of the coal used in the operation. Careful analyses had been made of the gases, and the quantity of ammonia present had been ascertained. The results, however, would have to be taken with a certain amount of reservation on account of the great difficulty of ascertaining the exact proportion of such a minute quantity of ammonia as was to be found in the gas. For every ton of iron made he had calculated that in the gases there was 0·11 cwt. of ammonia, which was equal to 12·32 lbs. to the ton of iron. When it was remembered that for every ton of iron made there passed out of the furnace 260,000 cubic feet of gaseous matter, or double this if the temperature was 480° Fahrenheit, it might be seen how difficult it was to ascertain with any degree of precision the quantity of ammonia present. If the figure 0·11 were increased one-half, making it 0·16, there would be one-half more ammonia than that accounted for. But the same difficulty also attended the practical man when he attempted to condense the ammonia, because of that quantity not more than one-half

Mr. Lowthian Bell.

was obtained by a most elaborate system of condensers applied to each blast-furnace. Therefore whatever the position of the coal might be with regard to nitrogen, and whatever might be the fact with regard to the power of the coke to retain nitrogen, it could scarcely be expected to deal with so vast a volume of material with the same amount of success as attended the chemist in his laboratory. With regard to the retention of nitrogen by coke, that was a matter which had not entered into his calculations up to the present time. But being anxious to ascertain in the blast-furnace itself the circumstances which regulated the production of ammonia, several experiments had been made which went to confirm the opinion enunciated by Mr. Marriott, that coke appeared to be sufficiently retentive of nitrogen gas. At all events it had been ascertained that at a distance of only 8 feet from where the air entered the blast-furnace, at which point the temperature was very high, there were in every thousand litres of gas 3·28 grams of ammonia. That varied slightly at different altitudes of the furnace, and at the ultimate point of escape there were 4·64 grams to the thousand litres. At the time he confessed he did not conceive that the generation of ammonia at that point was due to the nitrogen in the coke; he rather assumed that it was due in the first instance to the production of cyanogen salts. The Author was no doubt well aware that forty years ago Bunsen and Dr. (now Sir Lyon) Playfair were among the first who detected the presence of cyanogen salts in the gas of the blast-furnace. Since then little had been done in the investigation, until he resumed it at the Clarence Works. A great quantity of cyanogen salts existed in the blast-furnace, chiefly in the lower part of it, the amount decreasing sensibly in the higher portions. Its disappearance there was no doubt due in a great measure to the action which oxide of iron had upon cyanogen and its salts. Both the generation of cyanogen, and the generation of ammonia had been ascribed by himself, perhaps in error, to the direct action of the nitrogen of the air upon the potash and soda salts themselves, and he had arrived at that conclusion because undoubtedly there did disappear, from the atmosphere of the blast-furnace somewhere at that point, a quantity of nitrogen which he had a difficulty in otherwise accounting for. Those questions, however, required a great amount of time and patience before arriving at any very definite conclusion upon them, and the Paper would certainly lead him to reconsider the conclusions at which he had arrived. The operations of an iron-master were of course directed to the manufacture of

Mr. Lowthian Bell. iron; that was the object he had in view, and in the present state of the iron trade he need not say that if any by-products like sulphate of ammonia could be had at the moderate price the Author had stated, £15 per ton, they would be a welcome addition to the balance-sheet of most iron-makers whose acquaintance he had the honour of enjoying. But it would scarcely do on that account to interfere seriously with their operations. The observations he had made applied also to the manufacture of coke. Members were no doubt aware that the attention of metallurgists in the north of England had been directed to the obtaining of ammoniacal salts, not from the blast-furnace, but from the coke ovens themselves. They had been able to collect sulphate of ammonia to the extent of about 20 lbs. to the ton of coal used. They were, however, halting a little before adopting the new plan, encouraging as its prospects were, because they had not yet had it demonstrated that the coke obtained was equal to the coke at present in use. That might appear strange, but good coke in blast-furnaces was the first object of their attention, and although in a calorific point of view it would be impossible to prove any difference between what was called inferior coke and good coke, yet, in point of fact, in the blast-furnace there was a considerable difference. Unless they had coke which would not only support mechanically the great load it had to bear, but would also resist the power of carbonic acid to react upon it in the higher portions of the furnace, they would turn a cold shoulder to it. With regard to water-gas, hearing what had been done in Germany in that direction, he had gone to Essen, and had the opportunity of examining the manufacture of water-gas there, as applied to certain finer metallurgical operations. Its freedom from sulphur, its easy manipulation, and the high temperature it was capable of evolving, all fitted it well for the purpose to which it was being applied, namely, the welding of tubes intended for steam-boilers. At that time that was about the only object to which it was applied, and the difficulty of introducing water-gas for domestic and other purposes, in a great city like London, was insuperable. Gas, being applied chiefly for the purpose of illumination, it would be highly inconvenient to have two sets of pipes, one bringing the heating and the other the illuminating material. Since that time, however, the same gentlemen had devised a method by which the gas could be used as an illuminating medium, namely, by consuming it in the ordinary way in a gas-burner, and introducing within the burner a filament of an incombustible substance, magnesia being that commonly employed.

This, heated to a high temperature, became correspondingly highly incandescent, so that every cubic foot of the water-gas so treated was capable of affording as much light as nearly twice the same quantity of coal-gas. Mr. Lowthian Bell.

Dr. Tidy. Dr. MEYMOTT TIDY congratulated the Author on the numerous facts that he had brought forward, which, he said, were both valuable and interesting. He would limit his remarks to two points on which he entertained a somewhat different opinion, agreeing with the Author for the most part in the remainder. The comparative analyses of the coal used at gasworks and of the coke produced from it, were, as the Author had said, matters of very great importance. While he was assistant to the late Dr. Letheby, he made ninety-seven analyses of coal, eighteen of Newcastle, seven of Derbyshire, twenty-eight of Lancashire, eight of Scotch, and thirty-six of Welsh coals. In the case of Newcastle coal, the quantity of sulphur present varied from 2·85 to 0·06 per cent., the average being 1·24. In the Derbyshire coal the range was from 1·35 to 0·8, the average being 1·01. In the Lancashire coal the range was from 3·04 to 0·52, the average being 1·43. In the Scotch coal from 1·57 to 0·33, the average being 1·11. In the Welsh samples, the range was from 5·07 to 0·09, the average being 1·47. On seeing the samples Dr. Letheby remarked that these results, calculated on small samples, scarcely expressed facts. Of this Dr. Tidy was quite convinced, when he learnt from experiments conducted by themselves and others, that whether they used the coal containing a very small, or that containing a very large quantity of sulphur, the quantity of sulphur in the coal-gas varied very little. The explanation was not very far to seek. It was necessary to take a considerable quantity of the coal and to mix it thoroughly, in order to arrive at any just conclusions as to the quantity of sulphur present. The sulphur in coal was present in different forms, as free sulphur, as pyrites, as sulphates, and so on, and it was necessary to have a considerable quantity of coal to arrive at an accurate estimate of the amount present in any given sample. Since 1875 it had been his practice, when possible, to take a hundredweight of coal and to have it thoroughly powdered, taking average samples from the mass in which to estimate the sulphur. The results of a hundred or more analyses of coal from various places since 1875 had given an average of from 17 to 30 lbs. of sulphur per ton, showing a variation much less than that arrived at in earlier experiments. Whilst, therefore, he was quite ready to admit the enormous importance of the

Dr. Tidy. analysis of coal to the practical gas-maker, it was not sufficient to send the chemist a mere lump of coal picked out of a heap in order to ascertain the average amount of sulphur present. His own experiments had shown that after the volatile matter had been driven off from the coal, the quantity of sulphur in the coke was about one-half that in the original coal, so that about one-half of the sulphur had gone off in the gas. It was important to the gas engineer to know exactly how that sulphur went off, and during which part of the operation it was given off. Supposing that 10,000 cubic feet of gas were produced for every ton of coal, chemists had to account, in that 1,000 cubic feet, for half the sulphur that was in the original coal. Of that quantity one-half (or one-quarter of that which was in the coal) was given off, according to his experiments, during the distillation of the first 8,000 cubic feet, and the other half during the distillation of the remaining 2,000 cubic feet. That had been pointed out, many years ago, by Mr. Barlow, and the facts had been considerably elaborated by the remarkable researches of Mr. Ellissen at the Paris gasworks. Dr. Tidy had also had many opportunities of examining the question in considerable detail. He was quite ready to admit that it was advisable to keep as much sulphur out of the coal-gas as possible, although he had no desire to take any exaggerated view of its injurious properties and effects. No doubt the sulphur in the coal-gas, when burned, produced sulphurous acid, but that all this sulphurous acid got oxidized and produced sulphuric acid, might be theoretically the case, but practically was not true. If all the sulphur in the coal-gas became converted into sulphuric acid, there was not an article of furniture in a room where coal-gas was burnt that would stand a single month. He thought it was much better and easier to keep sulphur out of the gas than to take it out after it had once got in, and he was therefore in favour of materially limiting the quantity of gas produced. Engineers were no doubt tempted to produce a large quantity of gas per ton of coal carbonized. It looked well in the statement laid before the directors; but considering that during the latter part of the distillation the larger quantity of sulphur products were given off, and also a larger quantity of hydrogen which was not an illuminating gas, he thought it very advisable to limit as much as possible the amount of gas produced from a given quantity of coal. He did not wish to fix any quantity, but he looked with considerable suspicion on anything over 9,000 cubic feet per ton. With regard to the nitrogen and the amount of ammonia produced, he could add nothing to what Mr. Bell had so well said. On the subject of water-gas he was not in any sense at one with the Author of the Paper. The matter was very interesting from a toxicological point of view. The poisonous gas of coal-gas was carbonic oxide. He supposed he had seen as many cases of poisoning by coal-gas as any person present, and nothing was more certain that when a person died from the action of coal-gas it was from carbonic oxide, and not from carburetted hydrogen. Men were constantly working in mines where light carburetted hydrogen existed, and suffered no harm. The average of a considerable number of experiments gave from 5 to 12 per cent. of carbonic oxide in coal-gas. In ordinary twelve-candle gas the average amount was about 7½ per cent., whilst in cannel gas it was less. It did not add to the illuminating power of the gas, and being very poisonous, it was important. The colouring matter of arterial blood was of a brilliant vermillion, while that of venous blood was of a dark colour. The change from venous blood to arterial was brought about by the taking in of oxygen by a substance called hæmoglobin. Carbonic oxide was also absorbed by hæmoglobin, but it could not deliver it up as it could oxygen, and that was the source of the difficulty. As in asphyxia, people died because the venous blood could not be reddened, so in carbonic oxide poisoning they died because the brilliant red blood could not become venous. He admitted the importance of carbonic oxide for heating purposes. No doubt the heat produced by its combustion was nearly as great as that of hydrogen. One cubic foot would raise the temperature of 5,400 ozs. of water, or 16,500 cubic feet of air, 1° Fahrenheit. But the amount of carbonic oxide in water-gas was as great as from 34 to 38 per cent., and that fact alone would, in his judgment, knowing the inevitable leakage that occurred from pipes and mains, prevent its introduction into dwelling-houses for any purpose whatever, or even its being carried along the streets. He had made many experiments with it. A sparrow died instantly in a mixture of 98 per cent. of air and 2 per cent. of water-gas; whilst in a mixture containing only 1 per cent. of carbonic oxide, a guinea-pig died in a minute and a half. So intensely poisonous was water-gas, that, admitting its value as a heating agent, he was convinced it could be never used for domestic supply, since *Salus populi suprema lex*. Dr. Tidy

Dr. Henry E. Armstrong remarked that the Paper had greatly interested, but, at the same time, pained him; for it was very remarkable that in the year 1884 the Institution should be listening to a Paper by a private individual, particularly calling Dr. Armstrong.

Dr. Armstrong. attention to an extraordinary want of knowledge of what was going on during the distillation of coal. The Author's experiments were the first published experiments dealing with the question of the disposition of nitrogen. The results were certainly surprising and had attracted a good deal of attention. The question had hitherto remained far too much in the hands of the engineer. The manufacture of coal-gas had been purely an engineering problem. Perhaps no problem had been worked out in a more perfect manner, but he thought that far too much attention had been paid to the mere obtaining of the maximum amount of gas per ton of coal carbonized, the gas being of just such a quality as would pass muster, both with regard to its illuminating power and to the sulphur which it contained. Within the last few years the by-products had become of enormous importance. Coal-gas manufacturers were every day turning their attention more and more to the use of coke, to the ammonia produced, and to the coal-tar products, yet they were practically without knowledge with regard to the influence of the material and of the conditions on the character of the products; nor could they give a rational explanation of anything that was going on in the coal-gas retort. He knew of no coal-gas works having in their employment a chemist of repute. Many works, indeed, employed chemists, but their duties were those of mere analysts. The problems which came under consideration were of extreme complexity, requiring that the chemist should not only be a careful analyst, but should be fully possessed of the knowledge of the day, and be capable of extending that knowledge. But, in consequence of the penny-wise pound-foolish, system that had been adopted, gas-manufacturers were without the knowledge of a good many things that they ought to know. In future the coal-gas manufacturer would cease to be a coal-gas manufacturer pure and simple; he would have to turn his attention to the production not only of illuminating, but of heating gas, to the production of coke and of coal-tar products, and would have to produce something far richer in useful constituents, and far poorer in those which were of no value. A Paper like that under discussion ought to lead to an organized systematic study of the problem. It was no use, however, to leave it in the hands of private investigators, who could at best work with small quantities, and under conditions which did not give results that were certain to recur again in practice. Nor was it merely necessary to pay attention to the few points to which the present inquiry had been limited; it would be of great advantage if a well-selected committee were appointed to take

Dr. Armstrong.

questions of that kind in hand, and carry out the work on a large scale. It might involve considerable expense, but it would be expense well incurred, if a careful note were taken of the various things that went on when material of known character was operated on under known conditions. There was one point in the Paper to which he thought too much importance had been attached—the amount of nitrogen left behind in the coke and its recovery in the form of ammonia. Of course, it was important to recover all the ammonia given off as such, but he did not think there was much future for the recovery of the nitrogen left behind in coke in the form of ammonia. It would be much better to direct attention to the production of ammonia from atmospheric nitrogen. The coal supply would give out sooner or later, but it was to be hoped that the supply of atmospheric nitrogen would last a long time; and it only wanted a vigorous attack on the part of the chemist to solve the problem. The Author had also raised the question of the production of gas for fuel, and there, again, he thought there was a tendency in the wrong direction, viz., to the production of gas of the character of the so-called water-gas, which had no illuminating-power and a comparatively feeble heating-power. If gas were to be used at all as a heating agent in houses (and it was to be hoped that it might be in future), something was required with a much higher heating-power than the present coal-gas. He had had a good deal of experience in the use of gas for heating purposes, and he had come to the conclusion that practically with the present gas it was impossible to get a sufficiently high temperature and a sufficient amount of radiation to make it an economical mode of heating. Gas with a much higher heating-power, or something akin to the blow-pipe, was required. It was well known that the heating-power of gas increased with the illuminating power, and he thought it would be much better if, instead of trying to send out a useless illuminating gas, an attempt was made to distribute a far better illuminating gas than at present, and then there would be obtained a gas of greater heating-power. The hydrocarbons, rich in carbon, were good illuminating materials, had much higher heats of combustion than hydrocarbons like marsh-gas, which were destitute of illuminating-power. That might be realized on a large scale without much difficulty. Recently a process for coking coal and at the same time recovering much that was previously lost had attracted considerable attention, viz., the Jameson process, which consisted in carbonizing the coal in close ovens, extracting the gaseous products by means of a fan, and

Dr. Armstrong. condensing them. From the coal-tar point of view, the Jameson coking process was a complete failure, because it furnished a material which was in no sense coal-tar, but more like Scotch shale-oil; but by submitting the oil to a high temperature, as in the production of oil-gas so largely used by various railway companies in London, a gas was produced of very high illuminating-power, and therefore of high heating-power, together with a very considerable quantity of material very valuable from the tar-distillers' point of view.

Mr. Dibdin. Mr. W. J. Dibdin said he agreed in the importance of the statement that when coke was steamed, sulphur was eliminated; but unfortunately the Author had not stated to what extent that was the case. It was not clear from the Paper whether the whole or only a certain portion of it was separated. It would be interesting to know the Author's opinion of the popular idea that a higher percentage of sulphur was present in coke than in coal. Possibly it might be due to the fact shown by the Author's analyses, that coke contained only a small percentage of hydrogen, and consequently less moisture was produced during the combustion, and it might be that the sulphur compounds were oxidized only into the sulphurous, and not into the sulphuric state, and were thus more perceptible to the organs of smell than if they were in a higher state of oxidation. Coke also was burned in the open to a much greater extent than coal, and consequently the sulphurous-acid fumes would be noticed to a much greater extent. The question of sulphur in coal-gas, and the state of the sulphur after combustion, was one over which many battles had been fought. He agreed with Dr. Tidy, that the sulphur-compounds were mainly evolved during the combustion of coal-gas as sulphurous, and not as sulphuric acid. He had made many experiments on that point. He had passed the products of combustion through a double condensing apparatus, and he had only been able to obtain about 10 per cent. of the total sulphur present as sulphuric acid, while sulphurous acid in considerable quantities was passing off at the outlet of the apparatus. He was, therefore, confident that the greater part of the sulphur in coal-gas was converted into sulphurous acid during the process of combustion. On the other hand he looked upon the small amount of sulphurous acid obtained from coal-gas as by no means a great disadvantage. It was one of the most powerful disinfectants, and he questioned whether the health of the inhabitants of London would be much improved if all the sulphur, and consequently the sulphurous acid were eliminated from the gas-supply. It had been stated that the condition of the London gas-supply was very unsatisfactory in regard to the sulphur-compounds, but he failed to see how that could be the case as the average quantity of sulphur present was but little more than half that allowed by the Gas Referees. The Author had stated that the prospects of the water-gas industry had never been so bright as they were at the present time, and had expressed the opinion that certain industries would be benefited by the substitution of gaseous fuel free from sulphur for the coke now used. So far he agreed with him; but he hoped it would not be considered a satisfactory thing to introduce into dwelling houses such a dangerous gas as carbonic oxide, dangerous not only on account of its poisonous properties, but of its insidious character. A very slight defect in the fittings of a house with ordinary gas was of very little moment, because the escape would be detected by the odour of the gas, but with water-gas no such warning would be given, and a small escape might produce disastrous results. He deprecated any tampering with the illuminating power of the London gas-supply. Some people had advocated a cheap supply of poorer gas for the purpose of heating. One of the greatest defects of dwelling houses was the want of ventilation; but in order to obtain the same amount of illuminating power with a poor gas it would be necessary to burn much more of it, and consequently the evils arising from defective ventilation would be increased. With the atmospheric burners at present in use all that was required in the way of heating and cooking could be done, and he failed to see what further advantage would be gained by the use of carbonic oxide or water-gas, or poorer gas of the present character. If it was considered necessary, as no doubt it was, to improve the existing supply of gas, gas-companies should look to their internal economy. They should enter into competition with the coal merchant, and by so doing they would extend the use of gas very much more than at present, not only for illumination but for heating and cooking. The public would in that way secure greater cleanliness in the household and greater efficiency in the cooking apparatus. The Author had done good work in the required direction, and he had no doubt that the gas-companies would be among the first to appreciate and utilize his efforts. Mr. Dibdin.

Mr. J. A. Wanklyn remarked on a statement in the opening paragraph of the Author's Paper with which he did not think that those who had directed especial attention to the gas industry would agree. The Author had complained of the defective nature of the older analyses of coal, and stated that it would be generally Mr. Wanklyn.

Mr. Wanklyn. found that the carbon, hydrogen, and other things were given separately, but that the nitrogen and oxygen were lumped together. Every intelligent gas-manager had in his hands "King's Treatise on Gas Industry," the date of which was 1878, and on two consecutive pages of that book analyses were given of sixty-four samples of coal, the nitrogen being determined in every sample; so that as far back as 1878, and he had no doubt long before, those who were specially interested in the gas-industry had the means of knowing the high proportion of nitrogen existing in coal, of knowing that coal was as rich in nitrogen as was common wheaten flour; and the fact that in the gas industry only a small fraction of the nitrogen was rendered available had been known for many years. In his own laboratory it had long been a matter of common remark, and years before the Author had published anything on the subject the information had not only been current, but had been utilized, and the process for rendering more nitrogen available in the gas-industry had been patented by Mr. Cooper on the 2nd of June, 1882, which was notoriously before the Author's first communication. Owing to the work done in Mr. Wanklyn's laboratory, his attention had been for some time directed to the matters dealt with in the Paper, and he had not observed anything particularly novel in it. Most of the information given in the Paper had been known to himself. There was one figure to which he wished to direct particular attention, namely, 4·223, as expressive of the sulphur in lbs. obtained in 1,000 cubic feet of coal-gas. That meant that from every ton of coal 42 lbs. of sulphur existed in the gas, or, as explained by the Author, in the gas-tar and liquor and gas. Mr. Wanklyn had never known anything like that result. The other figures were more in accordance with his own experience. The meaning of the figure to which he had alluded would be that practically, the whole of the sulphur existing in the coal, except the merest fraction, was obtained in the form of volatile products in gas-making. If that statement were accurate, the result did not represent what commonly occurred in gas-making. Dr. Tidy had well put it in stating that when coal was distilled, about half the sulphur appeared in the volatile products, and about half stayed behind in the coke. From 100 tons of coal about 70 tons of coke were obtained. Part of the sulphur went over in the volatile product, and the coke that remained would be a little poorer than the coal that gave it. The question of what became of the sulphur in illuminating gas when it was burned had engaged the attention of Dr. Tidy and Mr. Dibdin, who had expressed the opinion that when the sulphur in gas was burned, the sulphur

Mr. Wanklyn.

went into sulphurous acid, and so remained. He believed, however, that there was ample demonstration that that was not the fact. It was true that sulphur burned to sulphurous acid, but in contact with water a further change took place, and it would be impossible to conduct the referee's operation for determining sulphur in gas if that change did not take place almost immediately. The fact that without using any oxidizing agent it was possible to obtain a certain amount of sulphuric acid by working the referee's sulphur-test, was a demonstration that the sulphur went rapidly into the state of sulphuric acid. The whole of the sulphurous acid was not immediately converted, but a considerable portion of it was; the remainder of the sulphurous acid was converted afterwards. He had very little doubt that if gas were burned in the middle of the room in which the meeting was assembled, very little of the sulphur would escape from it in the form of sulphurous acid; practically, the whole of it would go into the form of sulphuric acid, and attack any surfaces that it might reach. The sulphuric acid was produced in the presence of an immense quantity of water, and was necessarily exceedingly dilute; it was only owing to the circumstance that when sulphuric acid was dilute, and was allowed to evaporate spontaneously, it increased in strength, that its formation could be of any importance.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, Vice-President, said he hardly liked to address the members upon what was probably more a chemical than an engineering subject, but he might mention one or two points that the members might be glad to hear. Before referring to these points he should like to say a word upon the question of sulphur in gas. It was a curious thing, that when corporations acquired, by gentle compulsion, the undertakings of gas-companies, and introduced Bills into Parliament for their acquisition, all the clauses which bound the companies to get rid of sulphur, except in the state of sulphuretted hydrogen, were almost invariably omitted. One of two things must therefore be supposed, either that the corporations were indifferent to the health of their ratepayers (which it would be improper to suppose), or that the public need not greatly concern themselves about the sulphur in the gas, which was not in the condition of sulphuretted hydrogen. He could not help mentioning these facts, because when he had heard (as engineers often did hear) an energetic town clerk condemning a private company for being grasping and sordid, not caring for their consumers, and only caring for their own profits, he had been struck with the fact that the very persons who made these accusations, Sir Frederick Bramwell. and on the strength of them acquired property which was not theirs, were the first to get rid of the restrictions imposed upon the companies by the legislature. In regard to the subject of water-gas, he hardly liked to say a word in its favour after the very forcible oration of Dr. Tidy. He supposed that never since the days when Corporal Trim, let drop his hat in Mr. Shandy's kitchen on announcing the death of Shandy's eldest son, had a more effective percussion been made by any speaker than was made by Dr. Tidy, when he illustrated by that forcible and opportune blow on the table, how a bird would be killed by a certain percentage of carbonic oxide. Bearing Dr. Tidy's observations in mind, he feared he should be regarded as little short of a murderer if he said anything in favour of water-gas. It might interest the members to know that as long ago as the Exhibition of 1851 water-gas was tried at Vauxhall Gardens in the winter, and it was afterwards used for some years for a limited portion of Paris. The invention was that of Mr. J. Gillard, whose specification of 1849 he had before him. The inventor was perfectly alive to the undesirability of producing carbonic oxide, and he proposed a mode of making the gas by which the carbonic oxide would be the smallest, and by which the products would be principally hydrogen, with some amount of carbonic acid, which was got rid of by lime purification. The specification stated—"The system of deposing (*sic*) the steam on the surface of the coals, instead of having it to pass through them," was entirely new, and was never done before. The invention employed a horizontal gas retort, on the bottom of which lay wood charcoal. The retort was heated by an external fire. In the upper part there was a horizontal pipe with minute perforations on its under side, and steam was supplied to the pipe by a boiler. It issued from the holes on the under side, impinged on the surface of the hot charcoal, and, according to the view of the inventor, rose directly and went away by a pipe which led it to the purifier where the carbonic acid was got rid of. It was stated that very little carbonic oxide was produced by that process. The steam touched the hot charcoal and became decomposed into hydrogen and carbonic acid. It was not long in contact with the hot charcoal, it did not pass through it at all, and therefore the carbonic acid was not further changed into carbonic oxide. Sir Frederick Bramwell had made a report on the process, but he could not remember the analysis of the gas. He believed, however, that to a very large extent Mr. Gillard was successful in his object of making water-gas very slightly impregnated with carbonic oxide. The gas after the purification was principally

Sir Frederick Bramwell.

hydrogen. Of course it was practically a non-luminous gas, and the way in which it was rendered luminous was that mentioned by Mr. Lowthian Bell, and illustrated by an apparatus subsequently exhibited, viz., by the use of a platinum wick, which was rendered incandescent, and in that way a most beautiful and soft light was obtained. The inventor stated that the process of rendering hydrogen gas illuminating was "by causing a small jet of lighted gas to pass through a burner (the holes of which are very small) on a very thin platinum sheet, or also on a wick made of platinum web, having its threads excessively fine, and of a graduated section proportionate to the intensity of the pressure of the flame and of the burning hydrogen, a very powerful light is produced. The platinum threads become immediately incalescent (*sic*), and of a whiteness such that the luminous refulgence is extraordinarily brilliant." There could be no doubt that the gas gave a beautiful light, and that, carbonic oxide or no carbonic oxide, the products of combustion were such that persons could live in a room lighted with that gas with the same amount of comfort as in a room lighted by the electric light; whereas he certainly could never live in a room lighted by ordinary gas unless some provision was made to prevent the products of combustion reaching his lungs. He would therefore rather have Mr. Gillard's water-gas than the ordinary article of commerce sold by any gas-company or even by any corporation. Another phase of water-gas had come under his notice in Canada. Two years ago he was in Toronto, and on going over the gasworks he found that during the whole of the summer months, the city had been lighted by water-gas. The proportion of carbonic oxide was what might be expected without any special precautions being taken. The gas itself was hardly illuminating at all, but it was enriched by the vapour of petroleum, so that without any incandescent wick or anything of the kind it was a highly illuminating gas. On the day he was there (the 30th of September), the coal plant with which the gasworks had been originally fitted was being lighted up, in order to supplement for the winter months the water-gas supply. A previous speaker had objected to the extension of a gas which had a low illuminating power, because the great defect of dwelling-houses was in the ventilation, which he said would be impaired if more gas were burned. On the other hand it was suggested that if cheap gas were made it would be used for a variety of purposes, including its employment as a substitute for coal in open fire-places for heating. But what would be the effect on domestic ventilation if the public were to do away with their open fire-places, which, Sir Frederick Bramwell. however illogical and unmechanical, were, he believed, the salvation of the people of the country, providing as they did complete ventilation, complete because, if there was not proper ventilation, the result was a smoky chimney, a thing so intolerable as to demand instant attention and cure by adequate ventilation. In his view nothing could be more disastrous than a contrivance which gave heat in a proper economical and scientific way, but which would be accompanied (unless special precautions were taken in the original building of the house), by a decrease in the amount of ventilation.

Mr. West. Mr. JOHN WEST believed (with all respect for the statement of Mr. Wanklyn), that it was almost universally acknowledged that the Author had been the first to lay down systematically the weight of the nitrogen contained in coal. He had recently spoken on the subject to some friends connected with the Victoria University, who admitted that the Author's writings had set them thinking on the subject. The subject had been warmly taken up in Manchester, one of the great centres of chemical industry, and some practical chemists there had obtained extraordinary results in consequence of the information conveyed to them by the Author. At some large works lately erected in Manchester for the purpose of carbonizing coal for the utilization of the products, he had been informed that a very much larger weight of sulphate of ammonia was being produced than heretofore. That, he believed, was in consequence of the keynote struck by the Author. No doubt other eminent chemists possessed great knowledge on the subject, but it was not available to them as practical engineers. It had been thought that, on account of the extra quantity of sulphate of ammonia and other products now obtainable from coal, it might be of advantage to erect large works for the utilization of the products alone. He did not quite agree with the Author on the subject of water-gas. The matter had been taken up by the Society of Chemical Industry in Manchester, and a Paper had been read by Mr. Thomas Wright, of Nottingham, clearly showing that water-gas stood no chance whatever in England.[1] It might be said, however, that Mr. Wright did not take into consideration the quantity of ammonia contained in the coke. It would, no doubt, be a great thing if some of the smoke and obnoxious gases in large cities and towns could be removed, and he believed that some of the information which had been given in that room might be worked out by practical engineers, assisted, perhaps, by the chemist. No doubt the smoke-nuisance could be very much abated if coke was used for the production of gas for heating furnaces and other purposes. It was now being much applied in the district of Manchester, and the Smoke Abatement Society was taking a warm interest in the matter. The gas-department of the Corporation of Manchester had taken the subject in hand. All the boilers he had recently erected were heated with gas. If ammonia were obtained from the coke to the extent indicated, it might answer in Manchester to utilize coke for that purpose; but he did not think it would answer in London, where coke fetched a higher price than in Manchester. At present the price was only 3*s.* 6*d.* a ton in Manchester, or about 2*s.* per chaldron, whereas in London the price was 10*s.* per chaldron, so that it would pay better to sell the coke direct than to try to extract ammonia from it. He thought the gas-world was much indebted to the Author for his efforts, and there could be no doubt that a large number of manufacturing chemists in the Manchester district would benefit through the publication of his statements. Mr. West.

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[1] The Journal of the Society of Chemical Industry for Dec. 29, 1883.

Mr. FRANK LIVESEY thought that gas-manufacturers had good reason to thank the Author for his Paper, which he believed would lead to useful results, especially taken in connection with future investigations. The 70 per cent. of the sulphur left in the coke was, of course, lost, and of the 30 per cent. that went into the gas, one portion went in the form of sulphuretted hydrogen, and the other in the form of sulphur compounds and bisulphide of carbon. That which went in the form of sulphuretted hydrogen was not lost, because it could be recovered in a saleable form; the other portion was a nuisance in every respect not only to the gas manufacturers, but to the people in the neighbourhood of gasworks. It was supposed also to be a nuisance to the gas-consumer (but he did not know how its presence was detected), and doubtless it would be desirable to have all the sulphur in gas in the form of sulphuretted hydrogen, which was so easily removed. The Gas Referees allowed gas companies in London in winter to have 22 grains of sulphur per 100 feet of gas, and at one works that he had charge of the sulphur in the crude gas was at about that limit, but sometimes it was lower; so practically that gas required no purification whatever for sulphur compounds. At other works the amount ranged as high as 35 or 40 grains, and a good deal of expense and trouble was required to remove it; but when it was removed it was all lost. It would be well if the Author would continue his investigations and show how to make all the sulphur useful and saleable. With regard to nitrogen, the Author Mr. Livesey.

Mr. Livesey. stated that 15 per cent. of the nitrogen in the coal went to form ammonia, that 25 per cent. was unaccounted for (probably in the form of nitrogen in the gas), and that 60 per cent. remained in the coke, which was entirely lost. The Author had proposed one method of making the latter useful by turning the coke into gaseous fuel. If he could continue his experiments and show how to turn the whole into ammonia, that would be a very practical way of dealing with the matter. Mr. Wanklyn believed that it could be done in his particular way, but gas-manufacturers did not think that he had obtained the degree of success which he claimed. If more of the nitrogen of the coal could be put into the gas in the form of ammonia it would be recoverable and useful, but while it was in the coke it was lost. If chemists could show how to convert sulphur compounds into sulphuretted hydrogen, which caused no trouble to get rid of, it would be very useful for gas-companies, and would remove one of the objections arising from the supposed conversion of sulphur into sulphuric acid, and the resulting damage to furniture.

Mr. Dowson. Mr. J. E. Dowson said if water-gas was to be used at all it was doubtless necessary to face the question of carbonic oxide, and its known deleterious effects. Admitting as he did the danger attending the incautious use of that gas, he did not think it followed that so useful a fuel as water-gas should be set aside, merely because it contained a larger percentage of carbonic oxide than ordinary lighting gas. Whatever danger there was, there ought to be no unnecessary alarm, but due precaution should be taken to prevent harmful effects. It was well known that electric currents of high potential were dangerous if used incautiously; and the unfortunate guinea pig referred to by Dr. Tidy, as having died in a minute and half under the influence of carbonic oxide, would certainly not have lived as many seconds if it had been exposed to a current such as was frequently used for arc lights. There had also been abundant evidence given recently by Captain Noble, that there were many other substances, such as gunpowder, gun-cotton, &c., which were of a highly dangerous character if improperly treated. No one, however, dreamt of stopping their use on that account, nor would it be wise to stop the use of water-gas, simply because it contained an element of danger. It was extensively used in many of the largest towns in the United States, and in France and other countries where coal was dear, nearly every kitchen had its charcoal fire, producing this very carbonic oxide. Of course all necessary precautions should be taken, but a gas which was known to be useful and economical, ought not hastily to be set aside nor should the development of so important a subject as the production

Mr. Dowson.

and application of gaseous fuel be hindered. The further questions involved might be divided under two heads: first, the production and general distribution of gas for fuel, and secondly, its production in isolated factories and large buildings. As to whether existing companies were to supply water-gas for fuel purposes or coal-gas, carrying a higher percentage of hydrogen or marsh-gas than the ordinary lighting gas, that was a question which mainly depended upon the cost of production and the convenience of distribution; and those were points that could be best left to the companies themselves. But the important question remained of providing simple apparatus and a simple process for producing cheap fuel-gas at as quick a rate as it could be consumed in isolated factories, and other large establishments where heating or motive power was required. He did not know what kind of apparatus would be necessary to carry out the system advocated by the Author, but the public were certainly greatly indebted to him for his earnest endeavours to assist in the solution of the problem. Not long ago Professor Fleeming Jenkin had shown that the actual efficiency of some gas-engines was higher than that of the best steam-engines and boilers, and that their theoretical efficiency was higher still. From an economical point of view, however, a gas-engine indicating over 20 HP., when worked with so expensive a fuel as ordinary lighting gas was certainly very extravagant; but where a cheap gas could be used the results were excellent. A good illustration of that occurred about a fortnight ago, when Messrs. Crossley, of Manchester, and he made a careful determination of the actual fuel consumption per HP. with an Otto engine, worked with gas made in a set of his own apparatus. The whole of the gas-producing plant was placed on a weighing machine, with a loose water joint in the vertical pipe which conveyed the gas from the generator to the engine: in that way the weighing machine was free to move up or down, and readings could be taken at any time to ascertain the amount of fuel consumed, while the engine was kept fully loaded at a regular speed, and indicator diagrams were taken at frequent intervals. The engine indicated about 32 HP., and the actual fuel-consumption, adding 10 per cent. for waste and impurities, was barely 1·2 lb. per indicated HP. per hour. The engine had been in daily use about two years, and was not so well adapted to this gas as some now specially made for it, and in no way was anything done to favour the result. With that fact before them, and knowing how recent the gas-engine was, he thought engineers had good reason for recognising how large a question was involved in that of cheap fuel-gas, Mr. Dowson. especially where it could be produced in an apparatus requiring no more ground space and no more skilled labour than an ordinary steam boiler. Many other successful applications of cheap gaseous fuel could be mentioned, and he hoped that the question of carbonic oxide would be treated in a proper way, by taking all due precautions, and without giving way to anything like a scare.

Mr. Greville. Mr. H. LEICESTER GREVILLE remarked that there could be no doubt as to the presence of carbonic oxide in water-gas, but he did not think there ought to be any unreasonable scare on that account. The chemist in his laboratory had strychnia and various other poisons, which were perfectly harmless if kept in their proper sphere. It was the same with water-gas. There was no reason why gas-fittings should not be made perfectly sound. The Paper was one of great value, but the analyses were only of individual samples of coal, and if they were on a larger number of samples the ratios might possibly be altered. The proportion of sulphur remaining in the coke and going into the gas was very largely modified by the conditions under which the coal was carbonized. A great part of the sulphur found in the gas was evolved at the last part of the period of distillation, so that if a charge was kept in for a long period there would be a difference in the ratio between the sulphur in the gas and that in the coke. The figures given by the Author as to the proportion of cyanogen were low. Practically in gasworks the proportion of nitrogen as cyanides was much greater. The difficulty in utilizing the residual nitrogen in the coke by the manufacture of water-gas from a gas-company's point of view consisted of the dual system of mains which would be absolutely necessary, and he believed that the general opinion of engineers was, that the dual system would be so expensive as almost to place it out of the reach of practical engineering.

Mr. Davey. Mr. HENRY DAVEY said he was unable to speak on the chemical side of the question, but he could speak feelingly on the subject of the prevention of smoke, living as he did on the borders of a large manufacturing town. Manufacturers would get rid of smoke in the large manufacturing centres when it was profitable to do so and not before. As long as it was not profitable to the manufacturer to consume smoke he did not think that any number of Acts of Parliament would suppress the nuisance. Most practical engineers knew that under the existing system of raising steam it was not profitable to consume smoke, or at any rate the whole of it; that it was more economical to allow the larger proportion to escape into the atmosphere. He had watched with considerable interest the development of the distillation of coal, and the increased value of the by-products, and he looked with great hope to a time when, instead of consuming coal as was the practice in furnaces under steam-boilers a cheap gas would be used for the purpose. He was supported in that view when he found that in and around the manufacturing centres plant was being put down for the sole purpose of obtaining by-products; the gas, if used at all, being only used very imperfectly, and the gas, and often the coke, being almost thrown away. He knew of several cases where plant had been put down at collieries for the sole purpose of obtaining tar and ammonia. That being so, and seeing that a greater quantity of ammonia was being produced, and that there was every hope of a still greater quantity being obtained from a ton of coal, it seemed that the value of the by-products was continually increasing with the knowledge of the subject. As to the question of using gas for power purposes, he found that according to the consumption of illuminating gas by the best modern gas-engines (which were still in their infancy, and for large powers were still very defective), supposing 9,000 cubic feet of gas and ½ ton of coke to be obtained from the distillation of 1 ton of coal, the gas used in a gas-engine, and the coke in a steam-boiler, would together give two-thirds of the power that was obtained from a ton of coal in the best steam-engine, so that only one-third of the value of the coal and the cost of distribution of the gas would have to be covered by the sale of the by-products. For power and heating purposes the question had been raised whether the gas should be illuminating or non-illuminating. After a good deal of consideration he was of opinion that illuminating gas was the most suitable because of the great power for a given bulk. To distribute gas for power purposes the mains had to be kept as small as possible. In such a scheme as he had in view for supplying power to the manufacturing centres, it would be undesirable to have a bulky gas. In employing an illuminating gas, it would not be necessary to go to the great expense of purification which was required for illuminating purposes. That raised the important question whether such gaseous fuel should be supplied from the gasworks, or from an independent source—a question which it was rather premature to enter upon. But it was important to face the question of the cost of distribution, which in the case of water-gas would be very great indeed. He believed it took about three times the quantity for a given power, so that the mains would have to be three times the size. The Author had thrown out the idea that the coke, which would be produced in very large quantities if gas was manufactured on a **Mr. Davey.**

Mr. Davey. large scale for power and heating purposes, would require an outlet for its sale. The suggestion had been made to employ coke in the domestic fireplace, with the addition of a little gas to give it a flame, and to facilitate the lighting of fires. He had tried coke in his own house, and had found great difficulty in getting the servants to use it, because of the difficulty of lighting fires and keeping them up. That difficulty would disappear with the addition of gas, if it could be supplied at a cheap rate, say 1*s.* per 1,000 cubic feet, which could certainly be done in manufacturing centres like Leeds and Manchester. He had been assured by some of the members of the Leeds Corporation that the money they received for the by-products covered the whole cost of establishment, except that of distribution, for which alone the consumer was charged. That was very important, because if gas were supplied for power and heating purposes it would be supplied at such an enormously increased scale that the relative cost of distribution would be greatly reduced. Of course the practical fulfilment of any prophecy of that kind turned upon the development of the gas-engine. It would be of no use to have a cheap fuel for gas-engines, without the existence of gas-engines suitable for large powers. It should be remembered, however, that the gas-engine had been rapidly developed, and it was reasonable to suppose that in a short time it would receive a much greater development. At present engines were made of sufficient power for the general requirements of large manufacturing districts. In factories the modern system was not to employ one large engine to drive all the lines of shafting, but to put down small engines, one to each section. That enabled one section to be working when there was no work for the other parts. Therefore, even in the present state of the gas-engine, there was a large field for the employment of gas distributed for power-purposes. He wished that he could have given some positive information as to the higher development of gas-engines. He had been making some experiments during the last twelve months on the application of gas for power purposes; but though he had met with a certain amount of success, he was unable at present to put it to any practical form before the members.

Mr. Woodall. Mr. CORBET WOODALL said he was not sure whether Mr. Davey was of opinion that the better plan of distributing gas for power purposes was by making the ordinary illuminating gas as cheap as possible, and using it for light and power. That, at any rate, was Mr. Woodall's firm conviction. With the highest possible appreciation of the work that had been done by many gentlemen, Mr. Woodall. Mr. Dowson for example, following the lead of the late Sir William Siemens, he was satisfied that the work to be done by gas, consisting of carbonic oxide and hydrogen, was where the application was made immediately at the exit of the producer, and that there was really no hope for it as a gas to be distributed generally over towns, and used in a multitude of applications. There were some towns (he could scarcely believe that Leeds was one of them) in the north of England where gas was delivered into the gas-holder at a cost that was practically *nil*, the amount obtained for the by-products paying for the whole cost of the coal and labour; but even in such cases after interest had been paid upon the capital employed in manufacture and distribution it could not be sold to the consumer at much less than 1*s.* 6*d.* per 1,000 cubic feet. There could be no cheaper gas than that for distribution. If it were necessary to distribute three times the quantity that would be required in the case of illuminating gas to do a given amount of work, then these charges would be largely increased and "producer" gas became a very costly article. But apart from that, the nuisance of having another set of mains through the streets, necessarily of a large size, would be very great. A distinction should be drawn between the gas that could be used immediately at the exit of the producer, and gas distributed over the streets of a town for general use. He agreed with Dr. Armstrong as to the duties of gas-companies to call in the aid of scientific men more generally than they had done. After the appreciative remarks of Mr. Livesey, who represented one of the largest gas-companies in the world, it was hardly reasonable to suppose that the Author would have to work as he had done "for amusement," or that companies having the command of large funds would fail in future to call in scientific aid, of which, perhaps, they had not hitherto availed themselves as largely as their true interests required.

Mr. Foster. Mr. WILLIAM FOSTER in reply said he was afraid it would take too much time to refer to all the points alluded to by Mr. Lowthian Bell, but there were one or two matters personal to himself to which he should like to refer. In February 1881 Mr. Frank Livesey gave him a sample of Durham coal, and his investigations then began. In the summer of 1882, there were four chemists including himself, at the South Metropolitan Gas-works, and it was an open secret that coke contained a large amount of nitrogen. In the late spring of that year he had occasion to go to Cambridge, and in the course of conversation with Professor Liveing he had mentioned the results he had arrived at as to the large amount of nitrogen remaining behind in the coke. Up to

Mr. Foster. that point he had not read anything on the subject which gave him the slightest clue in the direction in which he was going. Professor Liveing told him with his usual candour that he had no special knowledge of the point, and that it was remarkable that it should have escaped such a worker as Mr. Lowthian Bell. Mr. Foster therefore the more carefully worked the matter out, and of course he had to guard himself against the possibilities of error. That nitrogen was present in the coke was known to Mr. Frank Livesey, and to his brother Mr. George Livesey. Among the chemists he had mentioned who were engaged on some scheme or other at the South Metropolitan works at that time was Professor Wanklyn, who had stated that the matter was well known, and that a patent had been taken out on the 2nd of June, 1882; but long prior to that period it was known among the officers of those works that nitrogen was in the coke. Subsequently to the 2nd of June 1882, but during the same month, he announced in the course of discussion the general results of his inquiries. All knew the high position occupied by Professor Wanklyn as a chemist, but in regard to matters connected with commercial relations it was not necessary to trace what had been done. Prior to the date that had been mentioned there was no great secret on the subject. A number of gentlemen had since taken out patents for the recovery of the nitrogen. The best proof he could give of the character of his researches was the commercial value attributed to them by the fact of patents having been taken out for working some process or other. He was not himself commercially interested in any of the schemes. His object had been to turn the knowledge to some use, and within the last few months he could point to twelve pieces of literature that had been directly the outcome of his earlier public experiments. The Tables that he had prepared were certainly novel. He did not think that any chemist in the room could point to a complete analysis of a sample of coke. It was not by any means the universal practice to give the amount of nitrogen in coal. Professor Roscoe had stated that coal contained 2 per cent. of nitrogen, but in no case cited did it come to anything like that figure. In the case of coal like cannel the nitrogen was very much less than in the other samples. He had lately received a sample of coal from Chili something like anthracite in appearance, yet having a composition closely approximating to Scotch cannel, and containing about the same percentage of nitrogen. Many of the analyses he had shown represented an average of four experiments each, and in no case did the amount of nitrogen come up to 2 per cent. The sulphur question was undoubtedly one of import-

Mr. Foster.

ance. Most of the facts had been ascertained before September last, and since then much information had been obtained from subsequent work which had not been recorded in the Paper, particularly with reference to the nitrogen unaccounted for. Chemists in Manchester had been working on that subject, and had shown that a certain amount of nitrogen existed in the tar. Every chemist knew that there were alkaloidal bodies containing nitrogen, but even the lately published results had not led him to modify his opinion that the greater proportion of the nitrogen under that head was still to be accounted for. With reference to Mr. Dibdin's question the loss of sulphur did not proceed *pari passu* with the loss of weight: the steam exercised some differentiating influence on the sulphur similar to that on nitrogen. As to the action of carbonic oxide, every physiologist was familiar with the statement of Dr. Tidy as to the red blood corpuscles. He had spoken of the horribly poisonous character of carbonic oxide; but he had not proposed to distribute heating-gas in houses. Mr. Foster had directed attention to the great need of gaseous fuel in large industries, and Mr. Lowthian Bell had stated that at Essen gaseous fuel was used for many of the finer purposes of steel-making. He was not previously aware of that circumstance, but the use was one which readily suggested itself. Sulphur could not easily be removed from the coke, but heating-gas could be made, and the sulphur be eliminated in a ready way. A great deal had been made of the action of carbonic oxide, but if water-gas was to be excluded, coal-gas might be excluded on the same ground. It might be said that with 10 per cent. or more of carbonic oxide in coal-gas it was like sitting on a volcano. Dr. Tidy had stated that he had probably seen more cases of carbonic-oxide poisoning than any other man, but could he point to six instances during the last year in London? When such cases occurred they were due to negligence in not regulating taps, to blowing out gas, or something in the nature of an accident. Was there any beneficent undertaking that did not entail certain pains and penalties? Hundreds of persons were killed annually by railway collisions, and chloroform killed nearly 1 per cent. of the persons subjected to it. The rose could not be grown without its thorn. He never went to Sheffield without thinking that something ought to be done in the way of economizing fuel. The smoke hung like a thick veil over the town simply in consequence of imperfect combustion. Mr. Davey stated that this indicated economy in the boiler, and could not be modified; but he could not agree with him in that respect. It was in this direction he had worked, and he thought that the most feasible

Mr. Foster. way of getting nitrogen in a valuable form out of coke was to turn it into gaseous fuel. In that way the sulphur could be eliminated and the nitrogen recovered as ammonia while producing a gaseous fuel which was smokeless. Little reference had been made to the question of the use of coke with coal-gas. The late Sir William Siemens had introduced a small apparatus (a kind of grate) in which he burned a small amount of coal-gas and coke, but that appeared to have taken very little hold upon the community. There could be no doubt that gaseous fuel of any description for domestic purposes ought to be used with solid fuel. With coke in a fire-place without bars, a slow combustion and a certain amount of genial effect could be secured by the presence of a small amount of coal-gas. Whatever might be done in the direction indicated, experimenters should never, as Sir William Siemens had said, lose sight of possibilities, but should try to attain to a certain maximum, although they might not at present be able to reach it. That was the spirit which had actuated him in working out the action of steam on coke, hoping thereby to obtain a fuel which he did not propose to distribute right and left indiscriminately, but which might first be used for great industrial operations, and afterwards for other purposes. He had lately had a letter from Bradford stating that a very important experiment would be commenced there with a view of supplying gaseous fuel to certain industries, and he hoped that the rival claims of patentees would not seriously jeopardize the undertaking.

Correspondence.

Mr. Beilby. Mr. G. Beilby observed, in respect to those divisions of the Paper in which the Author treated of the nitrogen of minerals, that this subject first engaged his attention twelve or thirteen years ago in the laboratory and works of the Oakbank Oil Company at Midcalder. The legitimate work of the company, as distillers of shales, &c., for the production of paraffin oils and ammonia, supplied a strong incentive to such researches, as well as exceptional facilities for their prosecution. He had elsewhere published some of the results of these researches.[1] When a person

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[1] "On the production of Ammonia from the Nitrogen of Minerals." The Chemical News, 1883, vol. xlvii., p. 221. "Young and Beilby's process for the treatment of Coal." Transactions of the Mining Institute of Scotland, 1883, vol. v., p. 158.

Mr. Beilby.

had been interested in, and worked so long at a subject like this, and especially when the interest and work had by so long a time preceded any general recognition of it by scientific and technical men, the feeling of proprietorship in it was apt to grow very strong. While pleading guilty to such a feeling, he was most anxious to recognise whatever was original and excellent in the work of others. It was, therefore, with all respect for the good work done by the Author, and recounted in the Paper, that he had most distinctly to challenge his claims of priority in this field of research. Twice in the Paper was this priority asserted; in the second case, at the conclusion of the Paper, the Author mentioned the names of Mr. Young and Mr. Beilby as among those who had, "since the publication of the Author's discovery," worked at the subject. For the present purpose it was sufficient to compare dates of publication, and their chronological order was as followed:—

12th April, 1881. Mr. William Young lodged a provisional specification, in which the following occurred:—

"The first part of this invention has for its object improvements in the system of distilling shale or other similar substances for the production of oil and ammonia, and consists in distilling the shale in such a manner that the oil shall be liberated and distilled from the shale at a low, uniform temperature, the remaining nitrogen being liberated at a much higher temperature as ammonia . . . . steam being employed for liberating the ammonia at the hot end."

18th May, 1881. Mr. Beilby applied for a patent, in the specification of which the following words occurred:—

"In working, however, according to any of the systems in common use, or publicly known prior to the date of the hereinbefore in part recited letters patent, the ammonia actually obtained is not nearly all that is obtainable from the shale subjected to distillation, as the shale, after all or most of the oil has been extracted from it, and when it is in the condition ordinarily known as that of 'spent shale,' is capable of yielding a considerable further quantity of ammonia if subjected to the joint action of a sufficiently high temperature and of superheated steam."

21st March, 1882. Mr. William Young and Mr. Beilby applied for a patent, in the provisional specification of which the following occurred:—

"The second part of our invention has for its object the obtaining from coal, shale, or other bituminous minerals when used for the production of illuminating and heating gases, of a largely

Mr. Beilby. increased yield of ammonia, and this is effected by subjecting the coke or residue of the coal or other substances, after it has parted with all or most of its hydrocarbon gases, to the action of sufficient heat and of steam, thereby obtaining a quantity of ammonia, which has hitherto been wasted, together with poor gases suitable for heating purposes."

4th July, 1882. Professor Foster made some statements as to his experiments on the nitrogen of minerals.[1]

December 1882. Professor Foster read a Paper on the same subject before the Chemical Society.[2]

The Papers read by Mr. Young and himself before various societies could necessarily only follow the applications for patents. The patents of Dr. Grouven of Leipzig, which dated from 1878, anticipated theirs as far as the general process of burning nitrogenous substances in an atmosphere of steam for the production of ammonia was concerned. They therefore worked certain of their processes under license from him.

The Author seemed to have paid very little attention to the proportion of the nitrogen of minerals, which was ultimately found in the condensed tars as alkaloidal substances. The tar or oil from bituminous shales and cannels sometimes contained 20 to 25 per cent. of the nitrogen of the original mineral, while from ordinary coals 5 to 7 per cent. of the nitrogen appeared in this form.[3] Mr. Watson Smith of Owens' College had recently published some independent observations on this point. In the Author's calculations relative to the production of ammonia and "water gas," a serious oversight had occurred; it was assumed that it was possible to obtain the largely increased yield of ammonia (90 lbs. of sulphate per ton) by consuming the coke according to the equation—

$$C + H_2O = CO + H_2.$$

But this could never be accomplished; the temperature at which this reaction took place was far above the dissociation point of ammonia, and only as the temperature of the reaction was reduced by the free use of excess of steam or other neutral gas, could any of the ammonia be saved. Such a reduction of temperature was always attended with the production of carbonic acid. In practice it was found that the reaction was more nearly expressed by the equation Mr. Beilby.

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[1] The Journal of Gas Lighting, 1882, vol. xl., p. 1081.

[2] Journal of the Chemical Society, No. ccxliii., p. 105.

[3] Chemical News, vol. xlvii., p. 221.

$$3C + 9H_2O = CO + 2CO_2 + 5H_2 + 4H_2O \text{ (excess).}$$

The true excess of steam employed, as indicated by the water condensed from the gas, amounted to from 400 to 500 gallons of water for every ton of coal gasified. This very large excess might be reduced considerably by the use of a neutral gas such as nitrogen, so that the employment of air along with steam effected a most decided economy, and at once brought the process within reach for many purposes for which it would otherwise be inadmissible economically. In a Paper read before the Glasgow section of the Society of Chemical Industry on the 11th of March, he had so fully entered into all of these questions that it would be out of place to do so here; but, as bearing directly on the Paper under discussion, the results of working the process on a manufacturing scale at Oakbank were here quoted. From a dross or slack containing (by analysis) nitrogen equal to 165 lbs. of sulphate of ammonia per ton, there had been regularly obtained for some months a yield of ammonia varying from 90 to 134 lbs. of sulphate per ton, or over 60 per cent. of the total nitrogen, and, after deducting the fuel used in the process itself, heating gas equal to at least 60 per cent. of the theoretical heating value of the coal had been available for other purposes.

Mr. Brunton. Mr. R. H. Brunton remarked that, in his Paper on "The Production of Paraffin and Paraffin Oils,"[1] he pointed out that the increased yield of ammonia obtained by the Henderson retort was due to the exposure of the shale to a current of highly superheated steam. The Author's experiments bore this out in a rather remarkable way, as he showed that not only did steam evolve the nitrogenous vapours, but it exercised an elective influence by acting on those which were specially of value for the production of ammonia. The general impression amongst practical men, up to within the last few years, had been that an increased temperature in the retort was the means by which an increased yield of ammonia could be obtained; but the amenability of the nitrogen to highly superheated steam was not fully appreciated. The Author's experiments also seemed to open up a large field for the Scotch oil companies, the working of which might possibly lead to highly remunerative results. He showed that from 1 ton of a certain quality of coke it was easy to obtain 140 lbs. of sulphate

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[1] Minutes of Proceedings Inst. C.E., vol. lxvi., p. 180.

Mr. Brunton. of ammonia. It might be assumed that a quantity something similar could be obtained from the spent shale of the Scotch oil companies. From shale, during its distillation in the old form of retort, 14 lbs. of sulphate per ton used to be obtained; by the Henderson retort an amount of 18 lbs. per ton was now obtained; and these figures, agreeing so nearly with the ammonia got from coal, gave an indication that the nitrogen in shale was somewhat similar to that in coal. But assuming that only half the above quantity, or say 70 lbs. per ton, was to be obtained from the spent shale, there were quantities of this material heaped up in mountains sufficiently large to form important features in the character of the country, quantities which might be estimated at from 10,000,000 to 15,000,000 tons, and which, at the above figure, would produce from 300,000 to 400,000 tons of sulphate of ammonia, this representing at the present market price from £5,000,000 to £7,000,000 sterling. These figures might offer some inducement to the Author to extend his experiments to spent shale, as, if there were any grounds for his assumption, a lucrative industry might be inaugurated.

Mr. Carr. Mr. W. Carr thought it likely that firms requiring a large expenditure of fuel for the production of power would become gas-makers on their own account; they would first convert their fuel into gas, and apply it in that form for raising steam, as being the more scientific and economical method of application. His reason for this opinion was, that gasworks would inevitably tend to cheapen ordinary gas, which would be used for both heating and lighting, and by small consumers for the production of power. Now to convert the coke into gas would require duplicate mains, and mains of very large dimensions, as it came to be used to any large extent. Take, for instance, a concern which consumed 30,000 tons of coal per annum for heating purposes. In the case of the adoption of gas, there would be required about 4,000,000 cubic feet per working day of ten hours delivered into the concern. Such an establishment existed within a mile of the Halifax Corporation Gasworks. The enormous outlay of capital in mains and gasholders to meet an extended consumption in that direction might be imagined. This one firm would require 30,000 tons of coke to satisfy their demands. The Halifax Corporation had for disposal about 20,000 tons. Gasworks could not go far in that direction, but could supply a cheap gas for all purposes requiring heat and light. On the other hand, private firms consuming much fuel would find it to their advantage to convert their fuel into gas first. Whether it would pay private firms to

Mr. Carr. recover the ammonia or not remained to be seen. He was rather afraid that it would interfere with another important consideration, that of regeneration. One reason why private firms were more likely to convert fuel into gas for themselves was, that they could adopt the regenerative principle, without which gaseous fuel would be a questionable advantage. Now whether it was possible to do this and still recover the ammonia was doubtful. He thought the gases must be cooled before the ammonia could be recovered, and, if so, much heating effect would be lost from the gaseous fuel, and the relative value of the heat lost and the ammonia gained would require to be determined.

Mr. Davis. Mr. GEORGE E. DAVIS remarked that the all-important question for the consideration of ammonia-manufacturers was whether by any process it was possible to eliminate the whole of the nitrogen in coal, as ammonia. He felt bound to say that the Author had shown how this could be accomplished. In Table IV. the Author had demonstrated that coal yielded amounts of sulphate of ammonia varying from 20·27 lbs. to 26·4 lbs. per ton of coal. He was inclined to think these amounts low, but even taking the highest there was great loss, as theory indicated that about 150 lbs. was possible, and he knew of no subject which offered so many inducements to the practical chemist as this one. True, about one-sixteenth of the total nitrogen of the coal was found in the tar, but, as the Author had shown, the bulk of it remained in the coke. The object of these remarks, however, was to point out how, from a sanitary point of view, the Author's idea of producing a water-gas would benefit mankind. He had no doubt that so-called fogs would never disappear until all cooking was done by gas, and heating by the combustion of coke; and he was convinced that if the public generally knew how economical this system was, even with gas at the present price, they would not hesitate to adopt it. Several of his friends in Manchester, after seeing the system, had adopted the use of gas-coke. In his own house not a particle of coal had entered for over twelve months; a very accurate account had been kept of the gas used for cooking and lighting, and the coke used for heating. The system of heating and cooking with coal he would call the "old system," while that of cooking and lighting with gas, and heating with coke, he would call the "new system." The comparison was as followed:—

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Mr. Davis.

——		Old System, per Annum.	New System, per Annum.
Lighting-gas . . .	3*s*. 2*d*. per 1,000 cubic feet	Cubic Feet. 51,000	Cubic Feet. 53,500
Cooking-gas . . .	,, ,,	None	34,500
Coal	9*d*. per cwt.	Tons. 22	None
Coke	7*s*. 6*d*. per ton	None	Tons. 8
Money value		£. *s*. *d*. 24 11 6	£. *s*. *d*. 16 18 3

So that it was much cheaper not to produce smoke, and it would be of incalculable benefit to the country if the Government would step in and compel householders to use gas for cooking, and coke for heating. Looking at it again from another point of view, that of the vitiation of the atmosphere by smoke and sulphur-acids, the basis of 20 grains of sulphur per 100 cubic feet having been taken for the sulphur impurity of coal-gas, and 1 per cent. of volatile sulphur to exist in coal and coke. Of course some specimens contained much more than this, and would make the comparison more odious:—

——	Old System.	New System.
Smoke	Lbs. the maximum	Lbs. the minimum
Sulphur from lighting	1·4	1·5
,, cooking	0·0	1·0
,, coal	493·0	0·0
,, coke	0·0	179·2
Lbs. per annum of sulphur evolved calculated as oil of vitriol . . . . approximate	1,482	546

Even with the old system of gas-making without special mains or fittings in a house, the use of gas for cooking was more economical and advantageous than the use of coal; and he had strong doubts whether water-gas made from coke could be distributed to consumers, as a special gas, at less money per unit of heating power than the existing coal-gas. It was in summer time that the use of gas for cooking offered the greatest advantages, and it was in the summer also that one-half of the ordinary gas-plant of

Mr. Davis.

the country was lying idle. His opinion was therefore this, that gas companies, instead of running after schemes for producing residuals, should make the most of the plant they already possessed, and offer more favourable terms to consumers to induce them to burn gas for cooking all the year round. There was no difficulty in the employment of coke for heating in ordinary fireplaces. There was no need for burning gas simultaneously with the coke; all that was necessary was a grate with a clay back, and to make sure that in lighting the fire the grate was well filled with coke, and a sheet-iron blower used to advance the combustion. If a water-gas could be supplied at about double the price per heat-unit now paid for coal, he was sure it would be universally and quickly adopted, and at the same time an immense benefit would be conferred on mankind by the reduction of sulphur acids in the atmosphere. If heating could be done with a fairly pure water-gas, it might be assumed that the quantity of oil of vitriol sent out into the air would be reduced from 1,482 lbs. annually to about 10 lbs., and no smoke would be produced in the latter case. He thought such a probability should be treated with all the importance it deserved, especially in or near large towns. If water-gas was to be made, the existing companies might be able to manufacture and distribute it at a profit; but, in his opinion, more attention should be paid to the distribution of coal-gas at cheaper rates for heating and cooking purposes.

Mr. Gandon.

Mr. C. Gandon remarked, with respect to the statement in the Paper, that coal-analyses did not usually give the nitrogen separately; that Tables would be found in King's Treatise on Coal-gas, in which the nitrogen, as well as all the other elements, were given for a number of different coals, and these Tables possessed the advantage over those in the Paper, that the names of the different coals were also given. A reference to the Tables in King's treatise would show the importance of this, as it would be seen that the nitrogen in Newcastle coal varied from 0·72 to 1·69 per cent. Also in Table I., accompanying the Paper, No. 3, described as a Yorkshire coal, contained only 0·55 per cent. of sulphur; while No. 5, also a Yorkshire coal, gave 2·00 per cent. of sulphur. Again in Table IV., No. 4, a Durham coal, produced 26·4 lbs. of ammonium sulphate per ton of coal, while Yorkshire coal, No. 5, yielded only 20·5 lbs. per ton, although there were some kinds of Yorkshire coal which in practice yielded more ammonium sulphate than the Durham coals. The Author's Tables were, however, interesting and useful, as they showed the distribution of the elements between the coke and the gas in a manner

Mr. Gandon. which had not before been done. The conclusion arrived at—that coke was preferable to coal as fuel because it contained less sulphur—seemed scarcely evident in all cases. It was true that a portion of the sulphur in the coal was taken up by the gas, and therefore the remaining coke contained less, but the coke from a ton of coal weighed only 12 to 13 cwt., and if a ton of this was required to do the same work as a ton of coal, it would in some cases contain more sulphur than the same weight of coal. As the Author observed, it was much to be regretted that the question of temperature of distillation had not received more attention, as it undoubtedly had considerable influence upon the results. It would also be interesting to know the quantities of the various products passing off at different periods of the distillation; no complete experiments seemed to have been made upon these points. When a charge of coal was put into a retort, it passed through all degrees from its normal temperature up to a dull or bright red heat, and different gases, or combinations of gases in varying quantities, must be evolved at the different temperatures; such variations were practically unavoidable, but a knowledge of what did take place might enable some evil results to be obviated; for instance, it was generally assumed that the proportion of sulphur compounds, other than sulphuretted hydrogen, was increased with high heats, and that the greater quantity passed off towards the end of the distillation, or when the retort was at its hottest. Referring to coal No. 1, the Author remarked that, "The coke which is produced is also considerably "richer in nitrogen than the parent coal;" but this seemed to require some explanation, as in Table IV. the nitrogen in the coal is given at 1·28, while in the coke it is only 0·661 per cent. The details of the production of ammonia from coke by means of steam were interesting and instructive, but it seemed doubtful whether it could in all cases be done at a profit. The coke from Scotch Cannel which had been experimented upon was of little value; but, assuming that similar quantities of ammonia would be obtained from the coke produced in and around London, and taking the Author's estimate that £40 worth of sulphate would result from the treatment of 100 tons of coke, the advantage of the operation appeared questionable; coke to the amount of 100 tons, at present London prices, would realise £75; but to obtain the ammonia all the coke was supposed to be consumed either for gasification or for fuel to produce the result, and it remained a question whether the fuel gases obtained would be equal to the difference between the values of the sulphate and the coke. It seemed also probable that the ammonia would be obtained

Mr. Gandon. in such a dilute state that the cost of converting it into sulphate would be much increased.

The desirability of extending the use of gaseous fuel for manufacturing and domestic purposes would be generally admitted; but although it might be advantageously used on the spot where it was produced without requiring to be cooled, stored and distributed, yet it was doubtful whether this could be economically done by distributing it from a manufactory in the same manner as illuminating-gas; such gaseous fuel might be produced at a cheaper rate than ordinary coal-gas, but its heating-power per cubic foot was greatly inferior to the latter, and in consequence of the enormously increased size of the storing and distributing plant that would be needed, it was doubtful whether coal-gas would not be the cheapest for most purposes: such water or producer-gas was also largely composed of carbonic oxide, the poisonous nature of which was a strong argument against its use in dwelling-houses. If coal-gas were more generally adopted for heating-purposes, the increased consumption would reduce its cost, and probably render it in most cases cheaper than a dual supply of lighting and heating gases, requiring duplicate storing and distributing plant, and duplicate fittings where the two were used. The question raised by the Author as to the difficulty of disposing of the increased quantity of coke that would be the outcome of an extended use of coal-gas for heating purposes was worthy of attention, as its value might thereby be reduced; but it should be possible to find means of increasing the demand of a smokeless solid fuel like coke, either by itself, or, as suggested by the Author, in combination with gas. Much had been done in this direction by breaking the coke sold for domestic purposes, and if this plan were more generally adopted, it would form one means of extending its use.

Mr. Hunt. Mr. C. Hunt remarked that in his Presidential address to the British Association of Gas Managers (now the Gas Institute) in 1881, he had referred to recent experiments, showing the influence of temperature and period of distillation upon the production of ammonia in gas manufacture, and suggested the desirability of further investigation of the subject. Since then the enquiry had been pursued with considerable success by the Author of the Paper, although the primary object of it appeared to have been to a considerable extent lost sight of. The Paper was mainly occupied with a statement of the results of experiments confirmatory of previous investigations. It had long been known that the nitrogen obtained as ammonia formed a very small proportion of the total quantity existing in the coal operated upon, and some

Mr. Hunt. years ago Dr. Grouven of Leipzig, demonstrated the practicability of its recovery in the form of ammonia from peat and other substances by the use of steam. More recently, Messrs. Young and Beilby had devised, and put into practice on a working scale, a process for the recovery of ammonia from gas-coke in combination with the manufacture of water-gas, identical with that experimented upon by the Author of the Paper. It might be admitted that the practicability of such a process as that of Messrs. Young and Beilby ought greatly to facilitate the introduction of gaseous fuel, if its economy were at the same time made clear; but the great cost of providing the necessary amount of steam appeared to be an obstacle to its success. This was acknowledged by Messrs. Young and Beilby, who, from motives of economy, utilized for the purpose the heat given off in the cooling of the resultant gases. He was somewhat surprised at the results obtained by the Author from his examinations of Durham and Yorkshire coals—the former according to him, yielding the largest quantity of ammonia, whereas in actual practice the reverse was invariably the case. Where Yorkshire or Derbyshire coals were used, from 30 to 35 gallons of liquor was no uncommon yield, while the yield from Durham coal seldom, if ever, exceeded 25 to 26 gallons of equal strength per ton. Could this disparity of results be accounted for by the temperature employed by the Author in his experiments? With respect to the employment of gaseous fuel in gasworks, the Author was scarcely accurate. The introduction of the Siemens system in the Paris gasworks dated, no doubt, as far back as twenty years ago; but it had also been tried almost simultaneously in two English works, namely, the Brick Lane Station of the Chartered Gas Co., and the Windsor Street works of the late Birmingham Gas Co. Mr. Hunt was responsible for its final abandonment at the latter works in 1875. A protracted trial, extending over several years, had proved its general unsuitableness for the purpose. Gas-makers had therefore held aloof from a system which, however successful in other industries, proved upon trial to be inapplicable to their own. At the same time they had not been slow to recognize the advantages of the regenerative system when presented in a sufficiently practicable form. It was the introduction, primarily by German gas-engineers, of continuous recuperation, as distinguished from the alternating system, which was identified with the name of the late Sir William Siemens, that more than anything else had rendered gas-firing both practicable and advantageous when applied to gas-retorts; while at the same time the most successful methods at present in

Mr. Hunt. use would be found, on examination and enquiry, to follow strictly upon the lines of a system known, as proved by the records of the Patent Office, nearly forty years ago. That in all this they had followed a safe course, was quite true, although not in the sense suggested by the Author of the Paper. The advocates of water-gas need not be apprehensive of indifference on the part of gas-makers to any properly matured scheme they might bring forward, and the possibility of the recovery of so large a quantity of ammonia in conjunction with the manufacture of gaseous fuel added greatly to the desirability of giving the subject the fullest consideration. He had already stated his opinion that there were no insuperable difficulties in the way of gasifying within the walls of the retort-house, for distribution if need be in the ordinary way, the whole of the coke produced in the manufacture of illuminating gas, and at an almost inappreciable cost for labour; but would the difficulties of the enterprise end here? Would there not then be a still more formidable one to encounter, namely the uncertain utility of the resultant gases for heating purposes? The Author had scarcely touched upon this all-important question, but the consideration of it could not be ignored—the fact being that there were few industrial purposes, and certainly none requiring a very high degree of heat, to which cold carbonic oxide or even water-gas could be successfully applied without recourse being had to recuperation, or some special means of concentration. Consequently new apparatuses in all directions would be needed before it could be successfully applied as a substitute for the open fire: and it was not to be anticipated that the change which this indicated could be otherwise than very gradual.

Mr. Jones. Mr. H. E. Jones said he should like to acknowledge the obligation which in his view the industry of gas-making owed to the Author for bringing forward the important fact which he had discovered of the valuable residuum of nitrogen contained in gas-coke, and further of his interesting experiments in the direction of developing that value in a commercial shape. It was well known that gas-coke did not bring its full fuel-value in the market owing to its being produced, not at the will of the gas-maker but by force of the demand for gas, so that it was not possible to suit the production to the demand. Any means, therefore, of realising the value of the product in a new manner must be of the greatest importance to gas-producers. He could not, however, quite follow the sanguine anticipations of the Author with regard to the means that he indicated for obtaining this end. He noticed in the first place that while submitting to a current

Mr. Jones. of steam coke maintained at a high temperature the rate of production of nitrogen rapidly fell off as the experiment continued. Thus, in the experiments recorded, about one half of the amount of ammonia was produced in ninety minutes at the end of the experiment which was produced in the first forty-five minutes, being in round figures reduced to a quarter the rate. This would mean, in pushing the process farther, an enormous expenditure of heat both upon the coke and upon the steam in proportion to the amount of ammonia obtained towards the end of the process.

And while it might be true that the coke would yield ultimately 140 lbs. of ammonium sulphate per ton, he feared it would not be easy in the later stages, and very far from economical. Passing to the estimate of the return of 4 tons of ammonium salts per 100 tons of coke, it would be obvious to those acquainted with the enormous discrepancy between the theoretical equivalent of fuel and the practical result obtained in steam-boilers, gas- and iron-furnaces, and other commercial applications of fuel, that the ratio of 14 parts of coke as fuel to 12 parts of coke converted would be very far from the commercial result. Judging from the loss in such furnaces as he had alluded to, and especially bearing in mind the great loss of gas furnaces owing to the high temperature that all parts of the retorts had to be maintained at, it would be safe to increase the ratios to 10 or 12 to 1 instead of 14 to 12. In this way the return from ammonia would be so small as to offer little encouragement of the process beyond what would be realised by the value of the water-gas. And in dealing with this part of the Author's suggestion he was bound to say that, apart from the objection of the danger to animal life accompanying the supply and the combustion of gas of this kind, there were serious difficulties of a commercial character in the way, the chief of which would be the necessity for providing a separate system of distributing pipes. Allowing for this gas not being universally used, and for the difference in its specific gravity as compared with coal-gas, he estimated that the interest upon the capital so employed would not be less than 8*d.* per 1,000 cubic feet of gas. To explain this he would point out that the average dividend charge of gas companies might fairly be stated, and had been stated by Mr. George Livesey, at 15*d.* per 1,000 cubic feet of gas sold upon an area. Allotting the proportion of this to the capital employed in distribution as distinguished from manufacture and storage, gave 5*d.* per 1,000 cubic feet upon ordinary gas. As water-gas had a specific gravity roughly 25 per cent. higher than that of coal-gas, it followed that pipes would have to be of a relatively larger

Mr. Jones.

calibre. Allowing for this and for the longer distances between the points of consumption, the total interest would come to 8*d*. as above stated. Adding for wear and tear of this plant 2*d*., and for the interest and wear and tear upon the plant for producing, together with labour, which by analogy with that for producing ordinary gas could not well be taken at less than 7*d*. per 1,000 cubic feet of gas, and allowing nothing for storage plant, which might nevertheless be required, the result was 1*s*. 5*d*. per 1,000 cubic feet of gas. Probably other charges for establishment expenses would have to be added, but without regard to these a cost would have been reached which, when it was borne in mind, as stated by Dr. Bunte, that the thermo value of this gas was only half that of ordinary illuminating gas, would admit of no gain to the consumer in substituting this gas for ordinary coal-gas at 2*s*. 10*d*. or 3*s*. per 1,000 cubic feet. He had endeavoured here to show the difficulties that lay in the path of any attempt to distribute a heating gas independently of and concurrently with a system of supply of illuminating gas, in reference to which last he should like to refer to the important statements of Dr. Bunte, which had been published within the last few days, of the comparative value for fuel purposes of the various kinds of gases which had been before the public and coal-gas.

The figures were as followed:—

	Units.
Heating-power per cubic metre of coal-gas . . .	5511·0
„ „ water-gas . . .	2813·0
„ generator- and water-gas mixed . .	1470·0
„ „ gas (Dowson)	1048·0

Coal-gas was, therefore, 5·3 times the value of generator-gas, and as nearly as possible double the value of water-gas. It would have been of interest if the Author had pursued his experiments up to the point of the exhaustion of the coke in developing ammonia, and had taken observations of the amount of coke consumed as fuel in converting a definite weight of coke to water-gas and ammonia.

Dr. Lunge.

Dr. G. Lunge, of Zurich, expressed his appreciation of the pains-taking and useful work which the Author had bestowed on the analyses of coal and coke, and upon the behaviour of the nitrogen contained in the same. Although he differed from the Author in some essential points, this only referred to his conclusions, whilst he saw no reason to doubt the accuracy of the facts which he had brought to light.

It was true, as the Author had stated, that most of the former

Dr. Lunge. analyses of coal and coke were incomplete, and he welcomed with pleasure his fuller analyses as a beginning in the right direction. But he did not consider six analyses, one from each coal-bearing district, as entitling to anything beyond provisional and conditional inferences. Much more material ought to be collected before coming to a firm conclusion in that respect. The very small quantity of ash found in all those six samples went far, in his opinion, to prove that they could not be accepted as representative of the average composition of their respective kinds. Much larger quantities of ash had been found by Mr. Pattinson and others;[1] but even apart from that it stood to reason that no single sample of coal could be said to fulfil that condition.

As to the results obtained by the Author on the behaviour of the sulphur in the coking process, he could not approve the statement that "it is generally accepted as an axiom that coke contains more sulphur than coal." Unless the Author referred to a vague popular impression, which he considered unnecessary to notice, he must demur to the above, and state his opinion that both among scientists and practical manufacturers the direct contrary was generally accepted, if not as an "axiom," at any rate as a fact. It was all the better that the Author's work strengthened the latter view. Among the most valuable results of the Author's inquiry were those obtained with reference to the disposition of the nitrogen of coal during destructive distillation. Hitherto it had been generally assumed that, apart from ammonia, most of the nitrogen was evolved in the free state, but little remaining behind in the coke. Whether this free nitrogen was evolved directly, according to the common opinion, or was formed subsequently by decomposition of ammonia, as assumed by the Author, was a minor question; but he thought the results as to the large quantity of nitrogen remaining behind in the coke must be granted, and in this instance he believed that the number of analyses sufficed for establishing that important fact. No doubt the experiments upon the evolution of ammonia from coke by the action of steam were highly interesting, and proved that there existed a possible reserve store of ammonia in a source hitherto overlooked. But it was another question whether this source was practically available, and this had not been decided by the experiments. Apart from the immense difference existing in every way between the conditions on the small and on the large scale, the Author had failed to show what entitled him to assume that the same quantity of ammonia as was obtained with 16 times the theoretical amount of steam should be got on the large scale when employing steam "not unduly in excess of the theory." The most serious criticism he had to exercise was that on the Author's calculations referring to the heating value of water-gas. He believed that both the amount of heat absorbed in producing it, and that evolved in burning it, had been wrongly calculated. He could not understand how the Author had not been startled by his own conclusion, that the theoretical value of water-gas as a heating agent, produced from 26 parts of carbon, of which 7 parts were assumed to be wasted "in conducting the operation on a commercial scale," should not be equal to 26 − 7 = 19 parts of carbon, namely, 73 per cent. of the quantity of coke consumed; but should be within 4 per cent. of the theoretical value of the whole of the coke used in its manufacture, including the 27 per cent. assumed to be wasted. This would upset the fundamental law of nature, that of the conservation of energy, which of course, was not set aside by the intervening process of water being decomposed and again recomposed from its elements! To simplify matters he should assume all the fundamental data as the Author had given them, although it might be doubted whether 600° Centigrade sufficed for effecting the complete reaction, and whether twice the theoretical amount of carbon was sufficient for carrying out the operation on a practical scale. In the first instance the calculation of the heat absorbed in producing the water-gas was wrong; for it contained the heat required for converting water to steam twice over, once directly in the item 18 × 626 = 11,268, another time in the "heat-units to effect the decomposition," which started from the datum 68,068 heat-units for separating a gram-molecule of water into hydrogen and oxygen. But this datum was only true when starting from liquid water, and hence already contained the above 11,268 heat-units. Consequently the total was not 55,679, but 44,411 heat-units, and corresponded not to 7, but to 5·5 parts of carbon for each 12 parts of carbon to be gasified. Doubling this, for practical purposes, as did the Author, 11 parts of carbon would be required to gasify 12 parts. These 23 parts, burned to carbonic acid in the ordinary way, would yield 23 × 8,080 = 185,840 heat-units. Now the Author stated 202,636 as the total heat-units developed by the combination of the hydrogen and carbon monoxide, resulting from gasifying 12 parts of carbon, that was, 9 per cent. above the theoretical value of the 23 parts of carbon consumed; but as 5·5 parts of this were wasted, really only 17·5 × 8,080 = 141,400 heat-units must be taken in comparison, and hence there would be a gain of 43 per cent. on the Dr. Lunge.

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[1] Transactions of the Newcastle Chemical Society, vol. iv., pp. 135 and 140.

Dr. Lunge. theoretical value indicated by the hitherto undisputed laws of nature. How the Author arrived at 202,636 he did not know; in reality the theoretical value of water-gas produced from 12 parts of carbon was:—

	Heat-units.
$CO + O = CO_2$ yielded	67,284
$H_2 + O = H_2O$ „	68,068
	135,352

From this must be deducted the latent heat of aqueous vapour at the ordinary temperature, which might be put at 600 without any great error, as the water produced in the combination always remained in the state of vapour; this must be multiplied with the molecular weight of water, and there would be obtained thus as the final result: $135{,}352 - 18 \times 600 = 124{,}552$ heat-units. This was just 67 per cent. of the heat-units produced by the direct combustion of the 23 parts of carbon; consequently the theoretical value of water-gas as a heating agent, according to the Author's own data, but calculated properly, was 33 per cent. below that of the coke used in its manufacture, not 4 per cent. as stated by the Author. How materially this difference must modify the practical conclusions and suggestions, based by the Author on his erroneous calculations, it was needless te follow out.

Mr. Smith. Mr. WATSON SMITH desired to refer firstly to the question of nitrogen unaccounted for, which was undoubtedly expelled from the coal in some form or other during destructive distillation. At a recent meeting of the Chemical Society he communicated a paper bearing upon this very subject, so far as the nitrogen existing in the tar was concerned.[1] He had made a series of determinations (1) of the amount of nitrogen in the crude tar itself, and had found 1·667 per cent. in the sample examined. Reckoning an average of 10 per cent. of tar to be obtained from coal in gas-making, this would mean 0·167 per cent. of nitrogen in the coal. Of course this nitrogen included that of some ammonia, existing as ammonia water suspended in the tar. (2) Of the amounts of nitrogen in the various fractions of the coal-tar, and in the pitch. It was found that in the various products of the coal-tar distiller, the nitrogen was pretty evenly distributed. It was further found that even hard pitch contained 1·595 per cent. of nitrogen, which on coking the pitch and allowing it to cool partially, at the same time allowing damp air to enter the still, was partly evolved as ammonia. Of course pitch contained a considerable amount of its nitrogen in the form of the substance which had been recently attracting much attention amongst organic chemists in Germany, namely, acridine. It was to the presence of this body that coal-tar pitch owed its acrid, stinging character, a property only too well known to the workmen who had to excavate it on a hot summer's day. Another extremely stable and high-boiling nitrogenous constituent of pitch was the imidophenylnaphthyl: (3) He had moreover determined the amounts of nitrogen in three kinds of coke prepared under different conditions. (*a*) Gas-retort coke (total exclusion of air, and high temperature for about six hours). (*b*) Beehive metallurgical coke (tolerably high temperature, admission of air to limited extent and lengthened duration of coking). (*c*) Simon-Carvès' oven coke (very high temperature, exclusion of air, and lengthened duration of coking). Amounts of nitrogen found—(*a*) 1·375 per cent.; (*b*) 0·511 per cent.; and (*c*) 0·384 per cent. Though different coals were of course employed, yet some comparison could be made, because the variation of the amounts of nitrogen left in coke when using one and the same process of distillation was not at all great. Taking this as granted then, it was seen that the effect of a strong heat of long duration, as in *c*, did act upon the very stable nitrogen compounds in coke, gradually dissipating the nitrogen. In the Simon-Carvès' ovens the four-ton charge lay in a vertically disposed and proportionately thin layer for about forty-eight hours, and exposed to a temperature of about 2400°. As regarded the dissipation of much of the ammonia first formed as nitrogen, and also of nitrogen in other forms, at the highest temperatures, he quite agreed with the Author. He desired, however, to call attention to a communication recently made to the Chemical Society by Professor William Ramsay and Dr. Sidney Young, on "The decomposition of ammonia by heat."[1] Thus, when ammonia was passed through an iron or porcelain tube, or a glass tube containing asbestos, the temperature at which it began to decompose was found to be a little under 500° Centigrade. In contact with a glass surface, the temperature at which decomposition began was much higher. Again, the influence of rate of passage of the ammonia gas, or in other words, of the time of exposure, was very great. Further, the nature of the surface of the containing vessel exerted an enormous influence on Mr. Smith.

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[1] "Note on (I.) the behaviour of the Nitrogen of Coal during Destructive Distillation, and (II.) a Comparison of the amounts of Nitrogen left in cokes of various origin." Journal of the Chemical Society, 1884, No. cclvii., p. 144.

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[1] Journal of the Chemical Society, 1884, No. cclvi., p. 88.

Mr. Smith. the amount of decomposition. By increasing the surface, the decomposition was greater. Whereas, on passing the ammonia through a plain iron tube at 780° Centigrade, total decomposition ensued, only 2 per cent. of decomposition ensued at 760° Centigrade, when the gas was passed through a glass tube containing copper wire, and only 1·72 per cent. of ammonia was destroyed on passing the gas through a hard glass tube filled with bits of broken small glass tubing, the temperature being 780°. From these experiments at all events it might be concluded that until the walls became covered and coated with finely divided carbon or gas coke, it might be expected that the red-hot walls of the old iron gas-retorts would decompose far more ammonia than those of the present fire-clay retorts employed in gasworks. It had occurred to him that this strongly decomposing action of iron at high temperatures on ammonia might be accounted for to a great extent by the fact of the ready permeability of red hot iron to hydrogen gas, the decomposing action being aided and stimulated by the rapid removal by diffusion of the hydrogen gas as soon as set free by decomposition of the ammonia, thus a new atmosphere being continually created. As regarded the use of steam to obtain the nitrogen from coke as ammonia, it seemed to him that if the treatment were to take place in the gas retorts, and the action of the steaming process neither proved deleterious as regarded the gas produced, nor the tar, that the matter was one of pounds, shillings, and pence, as the Author reasoned, and that a middle course would possibly be advisable, in which to gain the more valuable ammonia: a certain weight of coke might be sacrificed. Of course a process like that of Mr. Tervet, in which the coke treated with hydrogen gas would not suffer diminution, other things being equal, would be still more valuable. It seemed to him that good coke was more likely in the future to be preferred for general household use than gas. It must be remembered that the present household grates and fireplaces were most clumsy, ineffective, and wasteful arrangements, in comparison with properly constructed stoves, which might be used, and so as to combine economy with a cheerful appearance.

Dr. Spiegel. Dr. A. Spiegel concluded that some of the Author's experiments had been undertaken with a view of showing that the manufacture of water-gas might be made to pay better by the simultaneous production of sulphate of ammonia. It was interesting to learn the extent to which he had been successful. He wished to point out however that greater expense in the production of water-gas had to be incurred than the Author assumed. Owing to his overrating the heating power of coke, when made to furnish the temperature requisite for the formation of water-gas, he arrived at too low an estimate of the percentage of coke taken away as fuel. In assuming the combustion of 12 parts of carbon in oxygen to carbonic acid to be attended with the production of 96,960 heat-units, the Author had forgotten that 1 part of carbon produced 8,080 heat-units, provided the gaseous products of the combustion gave up all their heat, that was, cooled down to the initial temperature of the oxygen used. But since it was well known that the gaseous products of the combustion left the furnace at a temperature about 300° higher than that of the retort they were heating, none of the heat carried off by these gases was available. In addition to this, carbonic acid did not form the only part of these gases when the combustion was carried on in air instead of oxygen. Neglecting the fact that far more oxygen in the shape of air passed into the furnace than was actually employed to bring about combustion, four times the amount of the oxygen used up had to be heated in nitrogen to the temperature at which the gases left the furnace. The Author assumed that the formation of water-gas at a red heat presumed a temperature of 600°. Taking this for granted, the gaseous products of the combustion, together with the nitrogen from the air, left the furnace at a temperature of 900°. 12 parts of carbon required 32 parts of oxygen in the shape of 128 parts of air. They yielded carbonic acid 44 parts, and there were left, nitrogen 128 parts; so that the result was, heating gases 172 parts, which left the furnace at a temperature of 900°. Neglecting the difference in the specific heat of the various components of these heating gases, and assuming them to be equal to that of air:—$172 \times 0 \cdot 2375 \times 1 \cdot 41 \times 890 = 51,262$ heat-units which left the furnace for every 12 parts of carbon burnt. Deducting this number of units from that which 12 parts of carbon yielded when the products of its combustion were made to give up the whole of their heat, the heating value of the 12 parts of carbon which remained = 45,698 units. This was the theoretical number. When introduced into the Author's calculations the amount of fuel used up theoretically in making water-gas was found to be about equal to what the Author had allowed in practice. In practice the requisite amount of fuel would be found to exceed what theory demanded by 100 per cent., consequently to convert 12 parts of carbon into water-gas, 30 parts, instead of the Author's 14 parts, would have to be burnt as fuel. It was easy to see how this altered the case. It was useless to apply the results in order to effect a comparison between the heat produced by coke *per se*, and the heat produced by the same amount when consumed in making

Dr. Spiegel.

Dr. Spiegel. water-gas. Means had to be found to utilize the heat carried off by the gases leaving the furnace at a high temperature. It might prove practicable to employ it for heating the coal retorts at the gasworks manufacturing both coal-gas and water-gas.

Mr. Tervet. Mr. R. TERVET said that, the constituents of coal, when subjected to the action of heat in closed vessels, yielded to its influence; while at the same time other causes acted upon them and tended to change the place or position which they originally occupied. This movement and distribution of the constituent elements was partly the result of the heat upon the coal, and partly due to chemical affinity. With regard to the production of ammonia, the Author was careful to say that it was in some degree dependent upon the amount of nitrogen in the coal. Now, in the case of some coals, which contained a large proportion of nitrogen, their sterility in ammonia was due to their not possessing sufficient hydrogen for its formation, or to the fact that the hydrogen, although sufficient in quantity, was evolved under such conditions, and at such a time and temperature, as to preclude the formation of ammonia. It had been shown by Mr. Tervet, that the yield of ammonia could be increased to a very large extent by means of an artificial supply of hydrogen, either during the process of distillation or after its completion. It was essential, therefore, where the full advantage of the nitrogen was desired as ammonia, that such means should be employed as should enable it to take that form. The action of an artificial supply of hydrogen as an ammonia-producer, although very considerable, was limited to a gain in point of time; and in a process such as this, where all the changes were effected by contact, it was obviously necessary, for the exercise of the acting power, that to accelerate the formation, some endeavour must be made to render every particle accessible to the action of the hydrogen. It was well known that steam, when employed in sufficient quantity, had the power of eliminating from coal, &c., all the nitrogen in the form of ammonia; and that the elements of the coal were disunited by reason of the powerful attraction of the carbon for the oxygen of the water. New surfaces of the coal were thereby exposed to the action of the hydrogen, and the elimination of the ammonia was thus accelerated. Mr. Edmund Hunt, of Glasgow, had suggested the introduction of air into the retort, and this, which had now been extensively carried into practice, had the same effect but in a greater degree. Seeing, then, that hydrogen was so important a factor in the formation of ammonia, it was evident that the proportion of hydrogen which a coal was capable of yielding, was as important a consideration as its yield of nitrogen, and this was the more forcibly manifested on considering that a difference of 0·1 per cent. more or less hydrogen increased or diminished the volume of hydrogen to the extent of no less than 400 cubic feet per ton of coal; that was assuming the hydrogen to be evolved in the free state. Hence a difference of hydrogen in a coal, which might amount to from five to ten times the above quantity, must manifestly have a very considerable influence upon the reactions going on in the retort. Taking into consideration merely the ultimate composition of the coals employed by the Author, as tabulated in the Appendix to the Paper, it must be admitted that the results of his analyses conformed entirely to his opinion, namely, that the proportion of volatile matter had some influence upon the yield of ammonia; but he added, "clearly this of itself does not appear to be the full explanation." Without venturing any theory of his own, Mr. Tervet might be permitted to say, that there could scarcely be any doubt with regard to this point, taking into account the information furnished by the analyses in conjunction with that derived from practice. Mr. Tervet.

If the analysis of No. 1 coal was examined, it would be seen that that coal contained 8·70 per cent. of oxygen, and yielded 23·96 lbs. of sulphate per ton; while No. 5 coal contained only 3·66 per cent. of oxygen, and yielded 20·5 lbs. of sulphate per ton. He cited Nos. 1 and 5 because their analyses showed them to be so dissimilar; but he should prove farther on that they were almost identical. The same might be said of Nos. 2, 3, and 4. Further, the Author said, "Had the maximum results only been taken, No. 1 coal would have shown almost as high a yield of ammonia as No. 4." It was safe, therefore, to assume that by some slight modification of treatment, a still higher average result might have been obtained. Suppose, for the moment, that the greater proportion of the oxygen combined with the hydrogen to form water, it would then appear that there was present in No. 1 coal a condition which might be expected to favour in a very high degree the formation of ammonia; but it was known from experience that the production of ammonia was effected at three different stages, the conditions of which were different. The first took place when the coal might be said to be in the moist condition; the second occurred when the moisture had been expelled, and when the hydro-carbonaceous matter was undergoing decomposition; and the third was brought about by artificial means. At the be-

Mr. Tervet. ginning of the distillation of coal, &c., the quantity of matter in the act of undergoing transformation was naturally the greatest, and all the phenomena which accompanied the process, such as the evolution of gas, were most distinct at that time; but the yield of ammonia was much less than at the subsequent stages of the process, although relatively it was greatest in those coals which contained the largest proportion of volatile matter. As the distillation proceeded, the nature of the changes going on in the coal diminished; and when the greater part of the coal had undergone decomposition, the evolution of ammonia was produced in greatest abundance. Finally, the distillation and production of ammonia might be said to cease entirely long before the process could be regarded as anything like complete. So far, then, as the production of ammonia was influenced by the volatile matter, the point had been completely solved by the Author's analyses, which were so much the more valuable, as they had been made with a totally distinct object in view. But, on the other hand, coals possessed an unequal capacity in offering resistance to the disturbing influence of heat; or in other words, the permanency of a coal seemed to depend on a certain order and arrangement of its elementary composition. Now, if the several analyses of coal given in the Appendix were minutely compared, it would be found that, so far as the elementary composition was concerned, there existed a relationship between certain of them which might at first sight escape observation. To show this, he deducted the ash, moisture, oxygen, and the equivalent of hydrogen necessary to combine with the oxygen to form water. The analyses then stood thus:—

	1.	2.	3.	4.	5.
Carbon . . .	89·81	92·09	92·83	92·06	89·68
Hydrogen . . .	6·07	4·93	4·72	5·21	6·34
Nitrogen . . .	1·52	1·90	1·84	1·88	1·91
Sulphur . . .	2·60	1·08	0·61	0·85	2·07
	100·00	100·00	100·00	100·00	100 00

Thus it would be seen that Nos. 1 and 5 coals had the same elements united in the same numerical proportions; but in the two cases the nitrogen appeared to exert a most unequal power of offering or overcoming resistance. In No. 5 coal, it would certainly seem to be impeded by other forces, whose activity looked as if they gave preponderance either to its fixity or volatility. This was still farther shown by the fact that No. 1 coal, besides

Mr. Tervet.

yielding considerably more sulphate of ammonia per ton, also indicated a large proportion of its nitrogen as unaccounted for, and which had passed either into the gas or the tar; and further, the nitrogen remaining in the coke of No. 5 was 11·5 per cent. greater than that of No. 1. The foregoing observations were equally applicable, as an explanation of the deposition of the sulphur in coals. It was only by an exact estimation of the total sulphur, and an analysis of the ashes, that it could be learned what proportion might be expected to be variable, and what would remain constant. It was worthy of note that several of the coals, notably No. 1 and 5, did not contain sufficient mineral matter to combine with the equivalent of sulphur, and that they yielded the largest proportion of volatile sulphur. It might therefore be inferred, that the sulphur must be united with the carbon and hydrogen in some way at present unknown. He had carefully examined the figures tabulated in the Appendix, and had discovered many other points of analogy, both interesting and suggestive. The production of cyanogen was undoubtedly due to high heats, and its presence could only be accounted for on the assumption that there was an insufficient supply of hydrogen at the moment of formation, since it would otherwise have been resolved into ammonia. He had shown that all cyanogen compounds immediately yield ammonia in an atmosphere of hydrogen. He fully concurred with the Author that the nitrogen found in the free state was not evolved as such from the coal, but as ammonia, which latter was dissociated by the influence of heat and contact. The method of analysis adopted by the Author, although yielding strictly comparable results, would certainly have shown very different figures had the several coals been projected into a red-hot retort, especially in the case of Cannel coals. His own experience of Cannel coals and shales was, that the nitrogen had not the same degree of stability as in other coals, the least elevation of temperature beyond a certain limit being fatal to the formation of ammonia. He was strongly of the Author's opinion, that the deportment of the nitrogen in coals and shales generally, was to be found in the nature of the nitrogen itself, which might assume some allotropic modification not yet known. A preliminary notice of some experiments and observations made by Mr. Tervet on this point would be found in "The Journal of Gas-Lighting."[1] The imperfection of the present method of analysing coals for technical purposes had hitherto rendered it impossible to ascertain

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[1] Vol. xliii., 1884, p. 403.

Mr. Tervet. the true theory of destructive distillation. No new fact in regard to a more intimate knowledge of the functions of the constituent elements could be hoped to be attained from the results of the existing mode of analyzing; and technologists were thus deprived of what would otherwise be their best guide to their economical treatment. Any improvement was inconceivable without a deeper and better knowledge of the various decompositions which the coal underwent within the retort. With regard to the point of priority in the method of treating coals for the complete elimination of their ammonia, he might be permitted to state that, anterior to any precise steps being taken in the matter, he had completed a series of investigations, the results of which had been submitted to several scientific men, who advised that the subject should be pursued. Although the expression "incineration" was not his, the definition which he had given at the time was to "maintain the substance in a state of glowing combustion in an atmosphere of steam."

The Author's investigation and analyses cited all the conditions and observations essential to trustworthy experiment; and it was not too much to hope that, by means of such researches, and by the united diligence of chemists and technologists, we may ultimately arrive at a more advantageous method of employing our coal, and avoiding the present lavish and unnecessary expenditure of capital and power.

Mr. Townsend. Mr. H. TOWNSEND observed that it was in contemplation at Bradford to make an experiment, on a sufficiently large scale, to demonstrate the value of water-gas made from coke, for application to various industrial purposes. In a series of letters recently contributed to "The Journal of Gas Lighting,"[1] he had endeavoured to prove that if all the coke at present made in the gasworks at Bradford were to be converted into water-gas, and distributed through a separate service of mains, &c., and sold at 6*d.* per 1,000 cubic feet, an additional profit of nearly £20,000 per annum would be added to the Corporation exchequer, provided a market could be found for all the gas so produced, due allowance being made for the present value of the coke, the interest on the capital expended, and the working expenses so far as could be at present ascertained. He had also entered pretty fully into the sanitary aspect of the question, and had endeavoured to show that the poisonous properties of carbonic oxide had been greatly exaggerated, and that whereas water-gas, carburetted with petroleum, had been adopted for lighting purposes by no less than fifty towns in the United States, yet no increase in the death-rate had taken place in any of them; neither, so far as he had been able to ascertain, had any fatality occurred from its use, except in sleeping apartments. Accidents of a similar nature had also occurred with coal-gas, owing to defective fittings, and the carelessness of people who allowed gas to be escaping in their bedrooms *all night*. Should the experiment before alluded to be carried out, it was intended to adopt a simple method by which a strong odour would be given to the water-gas, and the proportion of carbonic oxide considerably reduced. Among the advantages which he had tried to prove would, in the course of a few years, be gained by the adoption of this policy, might be mentioned the following:—(1) The suppression of the smoke-nuisance; (2) the encouragement of industry by the supply of cheap gaseous fuel free from sulphur; (3) the gain to the agriculture of the country, by the production of cheap sulphate of ammonia; (4) the ultimate abolition of factory chimneys; (5) reduction in the consumption of coal; (6) reduction in the traffic on streets and roads; (7) substitution of cleanliness and economy for dirt and drudgery in domestic occupations; and (8) relief of rates from this new source of revenue. It remained to be seen whether these advantages were sufficient to outweigh the prejudice existing on the subject. Mr. Townsend.

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[1] Vol. xliii.

Mr. Foster. Mr. W. FOSTER, in reply to the correspondence, remarked that, owing to its great length, and the fact that many of the points raised had already been dealt with in the verbal discussion, he would limit himself to what appeared to be the most important matters. His observations in the Paper on the publication of his earlier results respecting the amount of nitrogen of coal which remained behind in the coke had called forth some quotations from specifications of processes having for their object the increase of ammonia in destructive distillation. The date of the specifications could not be disputed. Nor had he desired to imply by anything that he might have said or written that there were not chemists and manufacturers who, prior to June 1882, had great practical acquaintance with the subject. Yet the very specifications quoted, if discussed impartially, furnished more powerful evidence in favour of the Author's statements than anything that he had personally advanced. He believed that he was the first to disclose the scientific facts relating to the whole question of ammonia-production, and to make such knowledge available for public use. The publication of his earlier labours had thrown a new light on the subject, thereby giving it an interest which it

Mr. Foster. had not previously possessed. It would only have been fair if quotations from specifications bearing dates subsequent to June 1882 had also been given. It would then have been interesting to note how the latter had been altered in accordance with an advance of public knowledge.

The amount of ash in the samples of coal examined by the Author was small, as it should be, seeing that the samples were nearly all gas-coals of high repute. He was informed by the analyst referred to in the correspondence that the analyses in question were those of small coal used in manufacturing operations, and were known to contain more ash than the lump coal from the pits. The names of the pits from whence the Author's samples came had not been mentioned, for obvious reasons.

There was a blunder in his calculations of the thermal value of water-gas. The quantity 68,068 had been doubled. In taking 68,068 as the heat-units produced by the combustion of two parts of hydrogen in sixteen parts of oxygen, he was wrong in the way in which they were applied in the calculations. It was therefore his duty to acknowledge the correction made in the correspondence, while it was no less important that he should point out that the calculations given in the correspondence were still incorrect. He had therefore prepared a short addendum giving the data assumed, with the calculations, in tabular form.

In answer to the numerous criticisms respecting the amount of fuel needed to convert coke into a form of gaseous fuel, he would point to the experience recorded at Oakbank. He attached great importance to these data, and desired to give them all the prominence which they deserved. His calculations of the heat-values of gaseous fuel were purely incidental to the Paper.

The general results expressed in the Paper afforded material for considerable speculation, as was evidenced by the correspondence. On many of the points raised he wishes, for the present, to reserve his judgment, as he had pursued his inquiries since the Paper was written; and he hoped at some future time to give an account of his further investigations.

ADDENDUM.

The Author in the Paper calculated the theoretical amount of heat necessary to convert coke at a given temperature into gaseous fuel, allowing 100 per cent. more fuel for the purposes of "gasification" than was theoretically needed. Owing to errors referred to elsewhere he submitted the following figures, with the data employed.

Mr. Foster.

The following data were assumed:—

That the initial temperature was 10° Centigrade.
„ „ temperature of decomposition was 600° Centigrade.
„ „ water-gas was finally brought down to the initial temperature.

Specific heat of carbon	0·240
„ „ steam	0·480
„ „ carbonic oxide	0·245
„ „ hydrogen	3·409

	Heat-units.
Latent heat of steam	536
True heat of chemical combination of 2 parts of hydrogen with 16 parts of oxygen in producing vapour of water	57,924
Heat of combustion of 12 parts of carbon in oxygen to carbonic oxide	29,676
Heat of combustion of carbonic oxide (28 parts) in oxygen to carbonic acid	67,284
Heat of combustion of 12 parts of carbon in oxygen to carbonic acid	96,960

In considering the decompositions, 12 parts of carbon were acting on 18 parts of steam, and producing 28 parts of carbonic oxide and 2 parts of hydrogen. Thus—

$$C + H_2O = CO + H_2.$$

Taking the left side of the equation first, and regarding the total heat-units, there were:—

Carbon 12 × 0·24 × 590	= 1,699
Water to steam 18 × 626	= 11,268
Steam to 600° Centigrade 18 × 0·48 × 500	= 4,320
True heat of chemical combination of components of 18 parts of water	= 57,924
Total	75,211

Taking the right side of the equation, there were:—

Carbon to carbonic oxide	= 29,676
Carbonic oxide cooling down from 600° Centigrade to 10° Centigrade = 28 × 0·245 × 590	= 4,047
Hydrogen in cooling from 600° to 10° Centigrade = 2 × 3·409 × 590	= 4,022
Total	37,745

The difference between these two totals was 37,466, and represented the net amount of heat needed to effect the changes. Suppose this amount, which was theoretically necessary, to be doubled. It then became 74,932. To obtain this amount of heat, by the combustion of carbon to carbonic acid direct, required 9·3 parts of carbon nearly. Hence the "gasification" of 12 parts of carbon was attended with the use of 21·3 parts of carbon.

To compare now the thermal value of the water-gas with that of the whole coke used in its production, the latter being supposed to be burnt direct to carbonic acid.

Mr. Foster. The thermal value of the water-gas was :—

28 parts carbonic oxide to carbonic acid	= 67,284
2 parts hydrogen to vapour of water	= 57,924
Total	125,208

The thermal value of 21·3 parts of carbon was :—

$$21 \cdot 3 \times 8{,}080 = 172{,}104.$$

The ratio of this latter quantity to the former was that of 100 to 72. That was to say, if carbon was converted into water-gas and double the amount of fuel was used which was theoretically necessary, the gas obtained had a heating value of 72 per cent. of the whole carbon used in the process, supposing the latter to be burnt direct to carbonic acid.

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8 April, 1884.

Sir J. W. BAZALGETTE, C.B., President,
in the Chair.

The discussion upon the Paper by Mr. W. Foster, "Experiments on the Composition and Destructive Distillation of Coal," occupied the whole evening.

In pursuance of the notice on the card of the meetings it was resolved to adjourn for a fortnight, in order to avoid holding a meeting on the Tuesday in Easter week.

SECT. II.—OTHER SELECTED PAPERS.

(*Paper No. 1981.*)

"On the Elevation, Storage, and Shipment of Grain."

By WOODFORD PILKINGTON, M. Inst. C.E.

THE extended cultivation of grain for export in the United States of America over vast regions of fertile territory, has necessitated the invention and employment of special machinery and buildings for its reception, elevation, storage, and delivery or shipment.

As a case in point, the daily receipt and delivery of about 20,000,000 bushels of grain in one city alone, like Chicago, has called into operation a character of invention singularly adapted to the object in view, wherein the grain is treated pretty much as a fluid, and capable of being received and discharged in a similar manner. In this way the grain is pumped up and moved along in any direction, from the place of deposit or receipt to the point of delivery, through spouts, shoots, and orifices, by means of drag-belting and other appliances, and in troughs termed conveyors, the latter being sometimes used at distances varying from 200 to 300 feet from the main elevator building.

The impetus thus given to the grain trade in countries having a large area of production has lent an interest to this subject, as affecting the price of corn and other cereals in the markets of the world. This Paper, however, deals only with those mechanical and constructive details needful to the operations set forth in the premises, and not with the harvesting and transportation of grain in its relation to agriculture.

The grain-elevator may be aptly described as an oblong house, of varying dimensions, and of from 70 to 80 feet in height from the ground-line to the top of the cupola, divided vertically into bins.

The original method of storing in flats or floors having been found defective, gave rise to the self-delivering vertical bin system. These, again, suggested the hoisting-machinery for filling the bins by elevating the grain to any required height above the bin-level; first, for receiving the grain into the garners, which discharge into the weighing-receivers, and again for delivering the grain, according to the weighing capacity of the receivers, hopperful by hopperful into the bins. These bins are generally 60 feet high above their supports, and are capable of holding about 5,000 bushels each.

The elevator building is constructed on a series of piers or compound posts of sufficient strength to support it when filled, and the machinery employed for the purpose. They allow the passage in of railway cars, there being generally two sets of ways entering into each elevator building. The supports stand under the corners, and indicate the size and shape of each bin. The intermediate spaces are used for manipulating the bin-spouts and the levers or ropes for opening and shutting the bin-valves.

This mode of construction throws the weight of the building and its contents on the points of support, and hence the necessity of care being taken to have piles and piers of sufficient strength under the posts of a grain-elevator. In other respects, the construction, while being singularly unique, is most simple.

After the level of the bottom of the bins has been reached, planks, 6 inches by 2 inches, are laid horizontally across each other, constituting crib-work, which is fastened down by 5-inch spikes, and so bonded as to form laminated walls both for bins and house, dividing it vertically into square sub-divisions. An upper story is constructed above the four central rows of bins to contain the "garners" and "receivers," for weighing and distributing the grain into the bins. The grain is elevated up one or more of these bin-spaces by a belt and buckets in a tube called a "leg." This passes down below the entrance-floor into a receiving hopper termed a "boot" or "sink," into which the railway cars discharge their loads of grain. Each elevator-leg with its garner and receiver governs a certain number of bins forming its section of the house, generally twelve in number, representing from 50,000 to 60,000 bushels, so that an elevator house with twenty legs of ordinary size would have a bin-capacity equal to 1,000,000 or 1,250,000 bushels. About 20 HP. is required for each leg, so that a 1,000,000-bushel house would need from 400 to 500 engine HP.

Improvements in this particular direction have recently been adopted, each section being worked by separate engines stationed above, with steam supplied from a main boiler or boilers below.[1] This results in great economy, by dispensing with the huge india-rubber belts otherwise needful, which in large elevators are sometimes 160 feet long, and 3 to 4 feet wide, in favour of direct action, the steam-pipe being substituted for the belting. But the manner of working the elevator, the receiving and weighing the grain, and afterwards delivering it, is the same.

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[1] Three-cylinder engines of the Brotherhood type answer best.—W. P.

Each elevator-leg raises the grain into a garner, which, acting as a reservoir, discharges it again into a hopper attached to the platform of a Fairbank scales, capable of weighing from 20,000 to 30,000 lbs. of grain at a time. Every load is sent in turn into the bin then being filled by a revolving spout or swivel-shoot attached to the bottom of the hopper. When one bin has been filled in this way, the revolving spout is moved by a lever to another bin. The bins are numbered from 1 upwards.

The grain is weighed on receipt and again on delivery. That for delivery from the bins is discharged into the boot below, and re-elevated into the garner and weighing-hopper, whence it is passed into the shipping-bin, from which it is discharged into a ship's hold. If it has to be re-transported by rail, the ordinary discharging-spouts below the bins are used, with a special connection to fill the railway cars.

The cost of grain-elevators at the lowest rates averages from 40 to 50 cents a bushel all told; that is to say, an elevator constructed to hold 200,000 bushels would cost $100,000, one to hold 1,000,000 bushels, $500,000, and so on. This outlay may seem large, but the profits are equally so. The charge for receiving and storing grain is 1½ cent per bushel for one month, with ½ cent per bushel for every ten days beyond the first thirty; so that 1½ cent per bushel is generally the gross monthly earnings, from which working expenses have to be deducted. The gross earnings of a 200,000-bushel elevator, if kept in full work, would thus be $3,000 per month, or $36,000 a year. Setting one-half aside for working and incidental expenses, the annual profit is probably $18,000 upon an outlay of $100,000.

The Figs. in Plate 2 represent certain types of elevator and modes of construction referred to in this Paper, and seen by the Author in various places, such as Chicago, Boston, and Portland, which are some of the chief centres of the grain-trade.

Fig. 1 is a general plan of the bins in an ordinary elevator, having a capacity of 200,000 bushels, and a cross-section of the elevator legs, three in number. In larger houses these legs are arranged on each side, with two lines of counter-shafting, increasing in number with the length of the building. The position of the main driving-wheel, when continuous rubber-belting is used, is also shown, with the stairway surrounding the belt compartment.

This building is represented 50 feet by 100 feet, and is divided equally into thirty-four bins, and the half left by the belt and stairway. Two of the bins are divided into halves for shipping purposes, and one bin is divided into four sections to afford facilities for bagging. All the bins, except these latter, are of equal height—say 50 feet—and are partitioned off by planks 6 inches wide by 2 inches thick, laid flat, and held in place by 5-inch cut spikes, driven at intervals of 1 foot. The outside walls are of planks, 2 inches by 8 inches, fastened in the same manner. The bins are furnished with an iron ladder in each corner. The elevator-legs are independently braced, and are not connected with the building. The cross-sections, Figs. 2, 3, 4, represent different methods of construction, employed according to the position of an elevator, chiefly with reference to its foundation.

Fig. 2 shows a foundation of piling, driven to low-water mark, on which is built a stone wall generally capped with blocks of cut stone. The whole forms a foundation for the posts and braces as a support to the bins. The posts and braces are of hard pine, 12 inches square in cross-section, and the stringers, or sills, of oak, 12 inches square. This section also shows a main driver and engine-shaft, 9 feet in diameter, of 30-inch face, to carry a 26-inch belt to a pulley in the top storey, of 30-inch face and 7-feet diameter on a 5-inch shaft, clutching to a shaft 4 inches in diameter for driving the elevators. The height of the posts is 11½ feet; planks 4 inches by 12 inches being spiked between the braces at the top to prevent them from spreading; there is no mortise or tenon.

Fig. 3 represents the manner in which the grain or corn is taken from the car and finally reaches the bin. The grain is discharged by shovels or scrapers into the boot or sink (*a*), whence it is taken up by the elevator buckets and thrown into the garner (*b*), in the top storey. From here it passes automatically, at the will of the weigh-master, into the scale-hopper, and is thence, after weighing, conducted into any one of the twelve bins governed by this leg of the elevator. The scale-hopper is self-registering of the number of discharges, and checks the tally of the weigh-master.

The elevator-leg belt is 22 inches wide, and passes round a pulley in the basement, 20 inches in diameter and of 24-inch face, over a friction-pulley in the top storey of 24-inch face and 6 feet in diameter. This belting travels at a speed of 450 feet per minute, delivering about 100 bushels in that time. The capacity of the regular bins is about 5,000 bushels, or 300,000 lbs., so that they are generally filled or discharged in sixty minutes. A railway car has a capacity of 400 bushels, and can be emptied, including moving, in about ten minutes.

The object of the great height of an elevator is to retain the grain in the garner (the elevator running all the time) while weighing, and to have beneath all this structure sufficient depth to shoot the grain into the different bins. A space of at least 30 feet is needed above the sills. These are on a level with the top of the main bins of the building.

Fig. 5 represents a longitudinal section of the foundation, posts, and braces, under similar conditions with regard to locality; also a front view of the belting with buckets attached, the position of the garners, the weighing hoppers and scales, and the spouts for delivering to the different bins. It shows also the studding and bracing to the outer wall of the building above the bins. The garner holds 400 bushels, and the weighing-hopper 500 bushels. The discharging of the garner and weighing-hopper is accomplished by slides and levers, an operator on the second floor delivering, weighing, and discharging without changing his position.

The buckets on the elevator-belt are 20 inches long, 6 inches wide, and 6 inches deep. The counter-shaft on the upper storey is 4 inches in diameter, clutch-gear being used instead of coupling, in case of settlement of the building by shrinkage of the lumber.

Figs. 6 and 7 represent cross-sections of bin-hoppers at the point of discharge below. Here two sides are shown made of spruce planks, 6 inches wide by 2 inches thick, set up at an angle of 45° edgewise to the partition or bin walls, to a height of 2 feet above the stringers, and above that point 2 inches by 6 inches to the height of the bin. Half way on the incline of the hopper, 10-inch by 12-inch (*a*, *a*) ties pass through the partitions transversely. For a height of 1 foot above that running longitudinally of the building, planking 10-inch by 2-inch is used, and thence 8-inch by 2-inch to where the hopper-slope meets the bin-partition wall. The cross-section of hopper No. 2 shows the method of forming the other or opposite sides of the hopper, by planking on joists housed in transversely to the bin-partition walls. No spouts or slopes should have a less inclination than 35°.

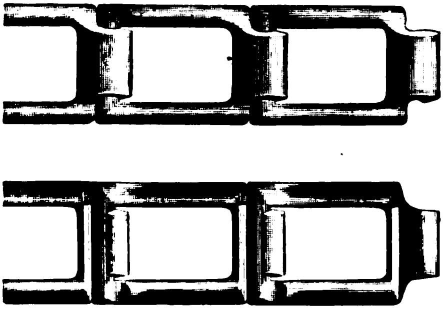
Fig. 8 shows the swivel shoot attached to the weighing-hopper of each elevator, and to the bottom of each bin. The upper casting is whole, the lower is divided into two sections, closing on the flange of the upper on which it revolves. The spout projection is of wood. It is hinged to the upper side, and may be of any length. The garner and scale-hopper have two sets of rods, each passing through them horizontally to keep them from spreading. The garner is 9 feet by 9 feet by 7 feet, and the scale-hopper is 10 feet cube. The bottoms of both are lined with sheet iron to the angle.

Fig. 9 is the end elevation of an elevator building, with delivery spouts for shipping grain, having a capacity of 6,000 bushels per hour. These spouts may be repeated, depending on the size of the house. The entrance way for cars for the delivery of grain, with the engine-house, &c., are also shown. The conveyor is sometimes extended along the wharves on trestles for hundreds of feet, the grain being led along a travelling bed of india-rubber belting in the conveyor trough, until it reaches the open hatch of the delivery spout then in use. Where circumstances are favourable, and on regularly made ground encased by dock walls, or where a suitably solid bottom exists in proximity to both water- and land-systems of transport, the method of constructing an elevator building may be successfully varied. Thus, after excavating the ground to the proper depth for the sinks or basement receivers, the entire floor may be covered with a bed of concrete. On this heavy footing courses can be laid, and the sink walls carried up, with the main walls of the building, to the level of the capping courses of the ordinary plan. This construction of foundation lessens the liability to settlement, and affords an opportunity for carrying some of the bin sides down to the ground. It not only steadies the building, but diminishes its height. By this plan many of the bin-hoppers or bottoms are inclined all one way, right or left, and shaped like a chisel instead of an inverted pyramid (Fig. 4).

Some of the changes in the latter design simplify the process of filling the bins as well as of receiving the grain. These improvements have been realized in consequence of the employment of the direct action of separate engine-power in the top storey or cupola of the building. But the method of building the house is essentially the same; the wooden laminated walls of boards crossing and re-crossing, forming vertical compartments of cribwork, cellular in character, to a height of 50 feet, is what constitutes its unique design and effects its object.

Whether, in countries where wood is expensive, a similar outline of form in iron could not with advantage be made to supersede wood, remains to be seen. The system itself has been hitherto self-developing. For the storage and delivery of grain it is adapted to situations facing the quay-walls of docks, where a large importation takes place by means of "dock-legs" attached to conveyors having special boots, capable of being lowered into the hold of a vessel. Thus, with a single-flight conveyor, grain may be elevated and conveyed to the bins with as much dispatch as it was at first discharged from the bins and delivered into the ship. For these purposes link-belting, coupling at each link, and running on sprocket-wheels in one or two strands, according to the size of the buckets, is now generally used in preference to india-rubber, as being less expensive and possessing many other advantages. Since the introduction of link-belting, conveyors running along the top storey of an elevator building have been made to do the work of the garners, carrying to any weighing hopper in use, and to any distance from the elevator leg (Fig. 4).

FIG. 4.



LINK BELTING.

The Author has recently had the opportunity of visiting the grain-elevators of Liverpool surrounding the "Waterloo Corn Dock," and also the large elevator in East Boston, constructed to store 1,000,000 bushels of grain. A few points in each are suggestive as valuable for further observation in connection with this subject.

In the Liverpool corn-stores, the whole range of these extensive buildings is constructed in flats, suited for the storage of flour or grain in bags; but unsuitable for grain in bulk. Labour-saving appliances, such as india-rubber belting running on concave rollers in combination with elevating buckets, are efficiently used, almost to the greatest extent possible under the circumstances; but the corn has to be handled by numerous gangs of men with shovels, both for filling and discharging the floors, which of necessity are never full, at an immense annual cost. Whereas by the vertical-bin system, without any space being sacrificed, the filling and discharging is self-acting. Nothing could be more simple or comparatively inexpensive than to adapt so much of these stores as is necessarily always required for grain in bulk, to vertical self-receiving, filling, and delivering compartments. The wasted energy in the Liverpool corn-stores, cannot surely be of less value in busy times than £10,000 a year.

In the new East Boston elevator, a novel method is employed for driving the upper line of counter-shafting. Instead of the india-rubber main belting, a vertical shaft is used of about 8 inches in diameter, extending the entire height required. This main driving shaft is built up in lengths of from 15 to 20 feet, coupled together with heavy wrought-iron couplings, the under sides of which are finished to run on supporting horizontal bearings laid on timber bridging-pieces housed into the wall and side of the bin-space, up which the vertical shaft passes. The whole of the machinery inside is driven by bevelled cogged-wheels fixed on the vertical and counter-shafting. By this means it is obvious that power may be employed at any desirable point on the vertical shaft. In this way, on an intermediate floor laid below the bottom of the bins, a horizontal shaft is run the whole length of the house, from which lines of counter-shafting are driven, giving motion as required to several appliances, by pulleys and belting. Among them are horizontal canvass conveyors on concave rollers, of sufficient width and strength on which to run sacks of corn to the shoots at each side for shipment or delivery after bagging; these run transversely on this floor level, the bags of corn being discharged like a string of sausages, laterally.

The most important function, however, of this lower floor shaft, is to empty the cars below into the boot or sink, whereby two men operating with mechanically-moved scrapers can do the work of six men with shovels, with a corresponding saving in time. This is effected in an ingenious way: on a counter-shaft over each boot or sink, are placed two pulleys, each carrying a friction belt, and working alternately fast and loose, by means of an automatic ratchet-lever, throwing the clutches, which move horizontally on the shaft between the pulleys, alternately into and out of gear; thus, as the rachet-lever falls over at each end of its arc, from side to side, as its sectors of motion reach either end of the spiral or worm gear fixed on the shaft from which it derives its forward and backward motion, each throw or fall of the lever shifts the double-clutch from pulley to pulley. The length of travel of the lever-ratchet on the worm gear is adjusted to a length of 15 feet, or half the length of a car of 30 feet. By this means 15 feet of rope attached to a staple in the bottom of a large scraper, having two handles at the top, is drawn along to the side door from one end, while a similar length of rope is released from its "reel" on the other. Thus while one man with his scraper is being hauled forward by engine power, his companion moves back, at the same time withdrawing his scraper, the motion of the winding-reels being reversed on each change of the automatic clutches, enabling him at great ease to do so. The effect is immense, a continuous flow of grain resembling a cataract being the result.

The entire work of this elevator is done by two compound horizontal engines of 600 HP. placed outside in a detached building; but although only one elevator-leg and car-discharging machine was at work, at the time of inspection, the engines were running at ordinary speed. This would show that the separate engine system for each elevator-leg and section of bins governed by it, is the most economical plan to adopt, as a separate engine placed on the intermediate floor under the bins, and over the car-ways, would do the work of discharging the cars and running the gear for conveying and delivering the bags of grain after filling.

For the rapid extinction of fire in a grain elevator, the steam-pipe of the separate-engine system is of distinct and obvious value, as the building can be surcharged with steam from valves workable from any convenient point.

The ordinary lift is now used, as well as the stairway shown on Figs. 1 and 5, for hoisting and lowering workmen.

The arrangements herein described for the elevation, storage, and shipment, or delivery of grain, must in time supersede, even in countries which are large consumers of imported grain, such as Great Britain, all travelling appliances, whether on land or water. It will rarely happen that a ship or steamer freighted with grain will not find it beneficial to berth alongside a regular receiving and storing elevator, capable of completing the work in one operation, and within the duration of a single day.

In loading vessels of 500 tons, or say 25,000 bushels, about ten hours' work of the elevator is necessary, as the insurance laws require two-thirds in bulk and one-third in bags for trimming the cargo; but in all cases the grain can be delivered faster than it can be stowed, and in a well-designed elevator it can be received again faster than a vessel can concentrate the grain around the boot or hopper of the dock-leg or legs inserted in her hold.

Steamers take from 20,000 to 60,000 bushels in bulk; these would find the construction of regular grain elevators in suitable positions on the quays of large docks of great value; whilst for the distribution or delivery of grain into railway cars from the steamers by the same process, and without delay, they would prove a fruitful source of revenue.

The Author has to thank Mr. Watson, elevator-designer and builder, of Chicago, and Mr. Cummings of Portland, U.S.A., for information connected with the subject of this Paper.

The Paper is accompanied by several diagrams, from which Plate 2 and the woodcuts in the text have been prepared.

(*Students' Paper No. 169.*)

"The New York, West Shore, and Buffalo Railway, and the Methods used in its Construction."[1]

By Peter Chalmers Cowan, B.Sc., Edinburgh, Stud. Inst. C.E.

This railway is described as a new double-track low-grade direct trunk line between New York City and the "Great Lakes." Its route is from Weehawken, opposite the City of New York, along the west shore of the Hudson River to Albany, through the Mohawk valley to Utica, and thence to Syracuse, Rochester and Buffalo. The main line is 425 miles in length. The terminal depôt at Weehawken is to be brought into communication with New York by large and swift ferry boats, and may easily be connected with the tunnel now in process of construction under the Hudson River at New York.

The railway is the first trunk line of similar length which has been built from the outset as a first-class double-track road, and its location, alignment and gradients have been carefully designed for the economical handling of a definite volume and class of traffic. Nearly all the other trunk lines in America were imperfectly located and cheaply constructed at first, and have had to be practically rebuilt. They have also in many cases been formed by the consolidation of small, tortuous roads, built originally for local traffic only. The doubling of a single-track railway is very expensive; for example, although there may not be much masonry to add, a large part of the wing-wall and abutment work has to be torn down and rebuilt, and the additional land needed is difficult to acquire.

The New York, West Shore and Buffalo Railway connects seven of the thirty-two cities in the United States, which have each a population exceeding 50,000.

The four existing trunk lines from the Atlantic seaboard to the West were opened nearly thirty years ago. Since then, the population of the country west of Pittsburgh and Buffalo has increased from less than 6,000,000 to more than 19,000,000, and the railway mileage from 6,000 to 53,000. For nearly length the "West Shore" Railway is in the State of N which contains one-tenth of the total population of tl States.

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[1] This communication was read and discussed at a Meeting of the Students on the 11th of January, 1884, and has been awarded a Miller Prize.

For one-third of its length the railway is level. The ing maximum grade" (Rankine's "ruling gradient") i per mile going east, the direction of the heavier traffic, a per mile going west, and these limits are only very exceeded for short distances. On the New York Central gradients of 86 feet per mile have to be overcome for co distances; on the Pennsylvania Railroad of 95 feet, an Baltimore and Ohio Railroad of 116 feet per mile. The in of these differences may be conceived when it is rememb

taking the tractional resistance on a straight level line as

resistance on an upward incline of 19 feet per mile is much.

Four-fifths of the railway is straight. The designed r curve was 4° (or one with a radius of 1,432 feet), bu difficult work along the Hudson River this curvature was in one or two instances. West of Albany there are curves sharper than 4°, namely, a 4½° curve in the city cuse, and a 5° curve 900 feet long. The curves are usu 1° to 3°. This straight alignment allows greater speed th otherwise be safely used, and besides saving power and tear, prevents the discomfort experienced by passenge trains are run at high speed round sharp curves.

The formation-level on the profiles is 18 inches below of rail, and shows the height of the slopes. From the to slopes to the centre-line the formation rises 6 inches for track and 3 inches for single track, to facilitate draina width of formation at subgrade is as follows:—

	Single Track. Feet.	Doubl Fe
On embankments	15	2
In earth excavation	20	3
In rock ,, ...	17	3

Figs. 1 and 2, Plate 3, show how these dimensions a sured. It should be noted that the ditches shown are of m size, and that in wet cuts the formation is widened and the are enlarged to suit the special circumstances. The doub widths detailed above are about 2 feet greater than the in New York State, and on the West Shore Railway the " way measures 7 feet 10½ inches. The additional clearance is necessitated by the great width of the parlour and sleeping cars used in America. The tracks of some of the important railroads in New York State are too closely spaced. In the summer of 1882, two cars on passing trains on the New York Central Railroad tore the sides out of each other.

To prevent a "rocking grade" at a summit, parabolic vertical curves are occasionally employed on the West Shore line. With a view, probably, to economise ballast, a general order was issued that the curve should be formed on the earthwork, but this instruction was not carried out, and the curving was effected in ballasting.

The vertical-curve practice was lightly esteemed by some of the most experienced of the division engineers, who considered a short, level line as good a connection between two opposite gradients as a vertical curve, and especially so with a low system of gradients such as that of the West Shore Railway.

		Hor. Ver.
Earth embankments, cuttings and ditches are sloped	. . .	1½ : 1
Earth and gravel excavations	,,	from 1 : 1 to 1½ : 1
Loose rock and "Hard Pan" excavations	,,	,, ¼ : 1 ,, 1 : 1
Clay and other slipping materials . .	,,	,, 1½ : 1 ,, 3 : 1
Solid rock excavations	,,	,, ⅛ : 1 ,, ¼ : 1

On "grading" the contractors are paid only for excavation, and the prices are understood to cover the entire cost of shifting the material, provided the average "haul" does not exceed 1,000 feet. Beyond this distance "extra haul" is allowed at the rate of ¾ cent. per cubic yard per 100 feet.

The prices per cubic yard for excavation on the West Shore Railway are:—

Earth	24 to 28 cents.
Hard clay	35 ,,
Hard pan	55 to 70 ,,
Loose rock	55 ,, 70 ,,
Solid ,, ...	$1 15 ,, $1 50 ,,
Excavation in water	80 ,, plus dry price.

A special allowance of 31 cents per cubic yard (additional) was made for the excavation of frozen earth, which was ordinarily about 3 feet deep.

"Earth" is defined to include soft clay, sand, gravel, loam, decomposed rock, stones, and boulders of less capacity than 1 cubic foot.

"Hard pan" includes quicksand, tough indurated clay or cemented gravel which needs blasting, cannot be ploughed with fewer than four horses, or requires two pickers to three shovellers.

"Hard clay" is material between earth and "hard pan," that requires one picker to two shovellers, and can only with difficulty be ploughed with two horses.

"Loose rock" includes all masses of stone between 1 cubic foot and 1 cubic yard in size; also all slate, shale and soft friable sandstone; also all rock in strata not exceeding 8 inches thick, which can be economically removed without blasting.

"Solid rock" includes all rock found in ledges or masses of more than 1 cubic yard, which in the opinion of the engineer may be best removed by blasting.

It is evidently impossible to make such a classification quite exact, and it may probably be most truthfully regarded as only an approximate guide to the resident engineers.

The following note is appended to the specification:—"The company does not intend that the contractor shall do work at less than cost on account of hardness of the material. In determining classification, the element of necessary expense in removing the material should guide the engineer in his decisions." The existence of this clause saved some of the contractors from heavy losses on "cuts" where the "hard pan" was very hard. The company ascertained the actual cost of the work from sworn testimony, and made extra allowances to balance this cost with the payments.

The gradients on the West Shore Railway have been governed but little by the old principle of balancing the "cuts" and fills, and borrow-pits have been freely used all along the line. In locating the railway, cuts were avoided as far as this was compatible with a short route, and the greater part of the line west of the Hudson River is on embankment.

Two great advantages of this formation are (1) that snow is easily removed from the banks; and (2) that the roadway is thoroughly drained.

Most of the heavy banks have been deposited from side-tipping cars. These cars are first run out on light single-track trestle-work erected on the original surface, and rising nearly to sub-grade. After this trestle-work has been filled in, the temporary track is shifted to the side till the double-track bank has been formed. The timber is left in the embankment and paid for by the company.

Near the city of Syracuse a bank about 30 feet high, 2 miles long, and containing about 800,000 cubic yards, was formed in this way from two borrow-pits, one pit 1,000 feet from the centre of the bank, the other 3 miles distant. Overhaul was not estimated in this case, and a sum of 40 cents per yard was paid for the excavation, haul and deposit of earth from the more distant pit, and of 35 cents per yard for earth from the other.

Steam-shovels were used, and averaged 600 cubic yards of material excavated per day of ten working hours. Small cars carrying 2½ cubic yards were employed; and two hundred of these handled by six 12-ton locomotives of 3-feet gauge, kept three steam-shovels busy in the more distant pit with a haul of 2¾ miles. The bank referred to affords a good instance of the necessity of allowing a liberal percentage for contingencies in preliminary estimates. The ground crossed is at the side of a deep lake, and was probably once part of the lake bed. Borings to the depth of 60 feet reached rock only at a few points, and showed soft marl underlying a thick peaty deposit. The region is a limestone one, and near the bank are numerous brine wells. One morning it was found that a part of the bank 200 feet long, which was almost at full height on the evening before, had sunk 12 feet into the ground, carrying the formation trestlework with it. On one side the ground was heaved in a fissured bank 150 feet long, 40 feet wide, and 7 feet above its previous level. This was the first of three or four similar sinkings. At one place it seemed almost impossible to fill in enough material, and a trestle 300 feet long, resting on piles, is now erected here. It is intended to fill this slowly after the railway is opened.

Most of the lower embankments have been made from small side borrow-pits and ditches, and, where possible, wheel-scrapers have been used as the cheapest means for shifting the material. In very favourable circumstances, 80 cubic yards per day of ten hours have been moved by each two-horse wheeled scraper, but 50 cubic yards is good work.

The scrapers are shovel-shaped pans, which are guided by hand, and filled by the pull of the horses. They are emptied by freeing the handles and allowing the scrapers to turn up as the horses go on. With most of the scrapers it is necessary for emptying that the point should catch the ground, but in some new forms this is not required. When the wheel scrapers are filled the load is lifted a few inches above the ground by an attached lever. For short hauls, "plain" scrapers with no wheels are sometimes used. Scrapers are useful in removing even hard pan or cemented gravel after it has been ploughed.

For finishing partly completed banks and for distributing ballast, a novel plough was sometimes used. The material is hauled on flat cars, and when the destination is reached the wheels of the cars are firmly braked and the material is unloaded by a large plough pulled along the cars by the locomotive. The plough is supported on small wheels, and is guided by a beam 6 inches by 8 inches in cross-section, bolted on the top of the cars. The rope used is a wire one, 1¼ inch in diameter. The ploughs are made in two forms, one form to throw equally on both sides, and the other to throw only to one side. The first form works very well indeed, but the great friction in the "side-ploughs" sometimes causes them to be thrown off the cars.

One of the resident engineers on the West Shore Railway devised a neat method for measuring tunnel work. He made a large wooden templet, shaped and divided like a common semicircular protractor, plumbed the centre of this above the centre line of the tunnel, recorded its height, and then measured from the centre of the templet to the points of the cutting, noting the angle shown by the number on the edge of the templet where the tape crossed it. In this way easily plotted notes were quickly and accurately got. The method, a direct application of polar co-ordination, is evidently general, but is best suited to tunnels or cuts with steep slopes. In contouring for borrow-pit measurements or other purposes, American engineers take levels along two sets of lines, dividing the ground into rectangles, varying in size according to the nature of the ground and the accuracy desired. Having these levels, contours are easily traced. Dumpy levels are almost unknown in the United States. "Y" levels are used, and these and the "transits" have, as a rule, erecting eyepieces, the small loss of light caused by the extra lens being disregarded as trifling.

Most of the levelling-rods have targets, which are used by the rodman on fine work, and always when the position of the instrument is changed.

For preliminary estimates of earthwork the volumes are obtained by scaling on the profile the cut or "fill" at the middle of each length of 100 feet, and taking the volume corresponding to the depth or height from a table, of level cuttings and fills. To allow for compression in banks, or slips in cuts, and the inaccuracy of the method, which is one of defect, 10 per cent. is added to the volumes thus obtained. In the division-engineer's office at Syracuse, a combination of scale and table was devised which saved much time in the estimation of earthwork. A copy of the scale-table for 28-feet formation is shown in Fig. 3. The point A is applied to the subgrade line on the profile, and then on glancing along the line on the scale which coincides with the original surface of the ground, the number of yards for the length of

100 feet is seen. With this table two persons, one giving and the other writing down the quantities, can estimate earthwork very quickly.

For monthly partial estimates, by which advances to the contractors are adjusted, the borrow-pits and cuts are measured, the cross-sections plotted, and the volume of a section taken as equal to the product of its length, and the arithmetical mean of the areas of its end sections. Although this method gives results in excess of the true ones, it is generally used on account of its convenience, and is approved by statute for final calculations upon public works in New York State.

For final estimates of earthwork on the "West Shore Railway," the prismoidal formula is employed, when the difference in the end heights would cause a sensible difference between the results of the various methods. Much of the labour of calculation is saved by the use of tables.

The Author would here state that, at the risk of tediousness, he has quoted freely from the specifications of the West Shore Railway, which he believes are representative of the best American railroad practice.

When the bank has reached subgrade level, the sleepers are placed upon it, and the rails laid down and spiked before ballasting. The apparent object of this is to enable the ballast to be distributed from trains. The track is ballasted with a layer, 18 inches thick, of clean stone, broken to pass through a ring 2½ inches in diameter, where such material is easily obtainable. Elsewhere, gravel free from earth, and stones larger than will pass through the 2½-inch ring is used. Experience on the Pennsylvania Railroad has shown that, except on hard bottom, broken stone alone is not so good as gravel alone for ballast, and that the best arrangement is a layer of broken stone on top of one of gravel. The ballast being brought forward, the rails and sleepers are lifted by levers and jack-screws, and the ballast is packed and tamped under them until the line is brought to grade. The track-laying specifications forbid the rails to be lifted more than 6 inches at one lift, and require both rails to be lifted together at opposite points.

The sleepers, or, as they are called in America, the cross-ties, number not less than 2,816 to a mile of single line, and are specified to be peeled, sawn square at the ends, squared on two opposite sides, not more than 4 inches out of straight, 8 feet long by 6 and 7 inches thick, and not less than 8 inches wide on each squared face. The kind of timber used varied much, and was generally that most easily delivered in the neighbourhood where the ties were required. As far as convenient, however, the hard and soft ties were laid in continuous stretches of one kind. The timber was not creosoted or otherwise fortified against decay. The prices for each tie delivered were:—

	Cents.
White oak, chestnut, or butternut	70
Cherry, and oak other than white	50
Yellow pine, hemlock, cedar, and larch	30

The rails are of steel, of the pattern called the Pennsylvania Railroad section (Fig. 4), and weigh 67 lbs. per yard. Flanged rails are universally used in America, and steel has now almost driven iron from the rail-market. In September 1883, steel rails were sold at $37, or a little more than £8 per ton at the mill.

The rail joints (Fig. 5) are supported ones, and are made with angle fish-plates of unusually great length and weight. The plates are 36 inches long, and are spiked upon three cross-ties, so that the joint has a very solid foundation. The rails are so laid that the joint on one side of the track is opposite the middle of the length of the rail on the other side. There is much difference of opinion concerning the relative merits of suspended and supported joints.

On the New York Elevated Railway the joints are supported, and the engineer who designed most o the ironwork for these railways told the Author emphatically that supported joints were the better for all railroads. On the New York Central Railroad some of the joints are supported and some suspended, but on all new work on the railroad the joints are now being made in the same way as on the "West Shore" Railway.

A simple form of check-nut for preventing joint-bolts from working loose has been found to answer well. In this, the screw of the nut starts exactly as in ordinary cases, but becomes gradually steeper on the sides of its grooves (Fig. 6.) The effect is to spring the threads of the bolt and secure an amount of friction which keeps the nut fast where it is first adjusted. The cutting of the varying screw of the nuts is effected by a modification of the screw-cutting lathe. The rails are fastened to each tie by two spikes, 6 inches long by ½-inch square, driven with 3 inches of "stagger" to lessen the chance of splitting, and to keep the ties at right angles to the track and preserve the gauge. To prevent the rails from "creeping," the edges of the fish-plates are notched to receive the edges of the spikes. This arrangement is recommended in preference to nicking the rail flanges.

The gauge is 4 feet 8½ inches. To allow locomotives with a long rigid wheel-base to pass easily round curves the gauge is widened ⅛ inch for a 2° curve, ¼ inch for a 4° curve and so on, ⅛ inch being added for every additional 2°. On curves, the lower rail carries the grade, and the superelevation is adjusted according to the following Table:—

		°	'		Inch.
Superelevation	for	0	30	curve	3/8
"	"	1		"	11/16
"	"	1	30	"	1 3/16
"	"	2		"	1 5/8
"	"	2	30	"	2
"	"	3		"	2 7/16
"	"	3	30	"	2 13/16
Superelevation	for	4		curve	3 1/4
"	"	4	30	"	3 5/8
"	"	5		"	4
"	"	6		"	4 13/16
"	"	7		"	5 3/8
"	"	8		"	6 7/16

The superelevation at the beginning and end of a curve is the same as at other points on it, and is tapered off on the tangents: for 1° curve in 50 feet; for 2° curve in 100 feet; for 3° curve in 150 feet; and for n° curve in n × 50 feet.

The specifications are very explicit regarding the allowance to be made for expansion of the rails, and require the contractor to provide a proper supply of iron "shims" differing in thickness according to the temperature at which the rails are laid. The thicknesses stated are: for 80° Fahrenheit, 1/6 inch; 70°, ⅛ inch; 60°, 3/16 inch; 50°, ¼ inch; 40°, 5/16 inch; and for 30°, ⅜ inch. The track-layers, however, certainly did not carry an assorted pack of shims in cloudy weather, and the joints have no striking uniformity in their clearances.

Wharton's patent switches and spring frogs are used on the main line of the West Shore Railway. These in a great measure prevent, by securing an unbroken bearing for the wheels, the jolting so destructive to wheels and rails, and so uncomfortable to passengers, which occurs with ordinary switches and frogs; they also minimise the dangers from the carelessness of switchmen.

Cattle guards are, like "cow-catchers," a special feature of railways in America, where there are no Parliamentary Standing Orders requiring all crossings to be provided with gates which close the road when the railway is clear, and *vice versâ*. Cattle guards are intended to prevent cattle from straying along the line from "grade crossings," and usually have the form of open pits or culverts, 4 feet wide, bounded by cheap masonry and bridged by wrought-iron I beams. A novel form of guard is shown on the standard plans of the West Shore Railway. It is a gridiron frame constructed of hard-wood scantlings of triangular section laid parallel with the rails.

On the West Shore Railway three classes of masonry for retaining-walls and bridge abutments were defined. First-class masonry is specified as rock-faced ashlar laid in regular horizontal courses not less than 10 inches, and not more than 30 inches thick, and having parallel beds and vertical joints. Stretchers are not less than 2½ feet nor more than 6 feet in length, not less than 1½ foot in width nor less in width than one and a quarter time their depth. Headers must not be less than 3½ feet, nor more than 4½ feet in length, not less in width than 1½ foot, nor less in width than their depth in course. The joints must not exceed ½ inch in thickness. The bed-joints are to fit close for at least 8 inches in from the face. Cement mortar is used in laying the stones, and each course is well grouted before it is covered. The backing is specified to be of good-sized well-shaped stones laid so as to break joint and thoroughly bond the work in all directions, and leave no spaces more than 6 inches. Any space left between the stones are to be filled with small stones well grouted.

All foundation courses must be laid with large flat stones at least 12 inches thick and with a bed of at least 15 square feet.

Second-class masonry is "broken range" rubble of superior quality with no stone less than 8 inches thick, and no joint wider than ¾ inch, and with foundation-stones with beds of at least 12 square feet.

Third-class masonry is defined as good substantial rubble work of sound stones laid on their natural beds, and sufficiently large to make well-bonded strong work. The stones in third-class foundation must have each a bed-area of at least 10 square feet.

By order of the State authorities first-class masonry was used in the abutments of the bridges carrying the railway over the canals. Second-class masonry was used in the bodies of other abutments, and generally also for the wings. Third-class masonry was employed for light highway bridges, in box-culverts, and sometimes for the wing-walls of railroad bridge abutments.

The coping projection was 6 inches, and the wing-walls are covered by steps 1 foot thick, 2 feet 3 inches wide, overlapping one another 1 foot, and projecting 3 inches on the face. The batter of the face was from ½ inch to 1 foot to 2 inches to 1 foot. The rule laid down in the specifications of the West Shore Railway is almost the same as that used on the Pennsylvania Railroad and the Erie Railroad; the thickness of the wall at all points to be four-tenths of the height to subgrade from the point considered. Generally, however, after fixing the thickness at the foot of the wall by this rule, the back was stepped off at the rate of about 1 foot to every 4 feet of rise, to give a thickness at the top under the coping of 1½ foot to 2 feet for a backwall, and 2 feet to 3 feet for a bridge seat. No "weeping holes" are provided in the walls on the West Shore Railway, and the banks were usually carelessly "dumped" behind the abutments. In consequence, a good many of the walls showed signs of distress by cracking and pushing forward; and the certain conclusion is that walls designed by the rules used on the West Shore Railway are too light, unless the mortar is allowed ample time to set; and unless the earth is carefully filled in layers behind them. Probably none of the walls would have shown weakness if the practice of piling loose stones behind them had been followed.

The culverts are of four kinds, namely, arch-culverts, box-culverts, open-culverts, and pipe-culverts.

Open-culverts are used, when considerable waterway is required through a low bank, and are formed of two third-class masonry walls 4 feet to 6 feet apart, spanned by wrought-iron **I**-beams.

For pipe-culverts, vitrified earthenware pipes were used under the first half of the West Shore Railway; but some of these failed, and cast-iron pipes were ordered for later work. The Pennsylvania Railroad has also abandoned the use of earthenware pipes under embankment; but the Author is not satisfied that earthenware pipes, fortified in soft places by concrete, are not as good as iron pipes. Some of the failures referred to were caused by careless insertion in frosty weather. Even with a liberal allowance of concrete, the earthenware pipes are much the cheaper expedient.

The timber used in the foundations was white oak, yellow-pine, spruce, or hemlock. The last variety was most commonly used for foundation platforms; it is cheaper than the others, and lasts well in wet positions. The piles are specified to be at least 12 inches in diameter at the butt, and 8 inches in diameter at the small end; and to be driven until they do not move more than ½ an inch under the blow of a 2,000-lb. ram falling 25 feet. This last requirement was never, or very exceptionally, fulfilled; the usual effect of the last blow was to drive the pile 1 or 2 inches. The piles are driven with the small ends down, and are not peeled. Piles of 60 to 70 feet in length, and of maximum diameters ranging from 15 to 18 inches, are commonly used in American work. In a few places where layers of hard materials had to be pierced, the piles were shod with cast-iron hollow conical points, 10 inches long, and weighing about 18 lbs. The load allowed upon each 12-inch pile, moving say 2 inches under the last blow, was 7 to 9 tons. The Author imagined Rankine's rule, allowing 200 lbs. per square inch of head, or about 10 tons for a pile 12 inches in diameter, a safe load for piles "standing in soft ground by friction," to be a very safe one, until he found that an abutment, weighing 1,400 tons, supported on two hundred and ten piles, each 1 foot in diameter, and 60 to 70 feet long, driven in white marl, had tipped back so as to increase the span by 6 inches, and that this wall and its companion had settled from 4 to 6 inches. The walls were 30 feet high from the bridge-seat to the timber foundation platform. The settlement mentioned took place before any trains had come on the bridge.

The crossing of the Seneca River a barrier well known on account of its difficulty, afforded much experience in soft foundations. The West Shore Railway effected this crossing by twenty spans of 50 feet, with pile foundations. The ground, which is mostly soft, varies irregularly. At one spot a pile 40 feet long would bring up on hard bottom, and split under the hammer, while 4 feet from this pile, another, 90 feet long, would be fully driven, and sink 6 inches under the last blow.

The New York Central Railroad crossed the Seneca River by 15-feet spans, and founded the piers on a continuous timber platform, sunk in a levelled trench. The piers have settled greatly, and very unequally, and blocking up is frequently necessary.

The foundation-platforms on top of the piles in the West Shore railway foundations at Seneca River, are formed by two layers of 10-inch by 12-inch timbers.

Wooden trestle-work is used for short distances on the line, as a temporary expedient where earth is difficult to obtain for embanking, and is of two kinds; namely, "timber-trestle," composed of ordinary trestle-frames, with "mud-sills" bedded in the ground, or supported on short piles; and "pile-trestle," in which long piles are partially driven into the ground, and the upper portions of them framed into trestle "bents."

The specification for cement is as follows:—"The cement must be of the best quality, of freshly burned and ground hydraulic cement, and must stand a proof tensile test of 50 lbs. per square inch on specimens mixed to a stiff paste, and allowed a set of thirty minutes in air, and twenty-four hours under water; and of 90 lbs. on specimens allowed a set of seven days under water. It must take not less than twenty-five minutes after mixing, to bear a weight of 4 ounces on a wire $\frac{1}{12}$ inch in diameter. Ninety per cent. of the cement must pass through a sieve of 2,500 meshes per square inch."

A large number of cements were rejected on account of insufficient strength; some because of their cracking after setting in water, and others because of their setting too quickly to fulfil the "wire-test." The tests were made with neat cement; but the Author favours the opinion that mixtures of cement and sand, similar to those used in masonry, should be adopted in testing. In the masonry 1 part of cement to 2 parts of sand is specified.

The cements commonly employed in America, are "natural cements"; that is, they are obtained by burning one kind of rock containing carbonate of lime and silicates. The strength of these cements is less than that of the artificially mixed English "Portland cement." It is claimed by the manufacturers of American cements, that a mixture of their cement with sand in equal portions, is stronger than a mixture of 1 part of English Portland cement to 2 parts of sand; and that considering the relative cost, it is more advantageous to use American cements.

The price of English Portland cement delivered on the West Shore Railway works, was about 4 dollars per barrel of 300 lbs. The price of American "natural" cements, which stood the tests detailed above, is 1·30 dollar per barrel of 300 lbs.

Few railway companies in America now build their own bridges. General specifications are usually furnished by them to bridge companies, and the structures are designed by engineers who make this work their special business, and see that their ideas are carried out in the "bridge shops." The strain-sheets and detail plans are submitted by the designers to the railway engineer before the bridge is constructed. On the West Shore Railway an experienced bridge-engineer was appointed to the duties of checking the plans, and superintending the design of the masonry. The masonry plans are designed in the railway offices.

The use of cast iron in railway bridges, and even in highway bridges, is rapidly being abandoned in America; wrought iron is almost universally adopted. Steel has, in a few instances, been chosen, mostly for compression members on small details. This is due to the fact, that as yet, steel of sufficiently uniform character can only be obtained at prohibitive prices. It is therefore neglected, except in such special cases as very long spans, where the weight of the girder must be minimised.

Wood is only used in the bridges in the form of sleepers and extra guard rails. The floor beams and stringers are made of built or rolled wrought iron beams. The specification of the West Shore Railway says:—

All parts of the bridge superstructure shall be of wrought iron, except bed-plates and washers, which may be of cast-iron. This rule has not, however, been rigidly adhered to, as some of the pin-connected bridges, varying in span from 80 to 220 feet, have been erected by the Phœnix Bridge Company, who employ iron castings to form the joints between the posts, or vertical members, and the upper boom.

This practice is due to the fact that the Phœnix patent column, an economical form of strut, is not easily connected without the use of cast-iron joint boxes. The Phœnix column is built up of circular segmental plates, with external radial flanges riveted together to form the pillar. These columns support the highest sections of the New York City Elevated Railways. The Phœnix column, in common with other pillars of closed cross-section, has the objection that it cannot easily be painted over all its surface after erection. The importance of this defect is now well recognized, and most of the American bridge builders now use angle and trough-shaped sections braced with light lattice bars for the struts in their trusses.

The following methods of bridge construction are employed on the West Shore Railway:—

For spans up to 20 feet, rolled I beams.
„ „ from 20 to 50 „ riveted plate girders.
„ „ „ 50 to 75 „ riveted lattice girders.
„ „ „ 75 feet and upwards, pin-connected trusses.

The widths of the bridges and the arrangements of the floors are shown in Plate 3, Figs. 7, 8, 9 and 10. They are all about 2 feet wider than those commonly used, on account of the tracks being spaced 13 feet apart from centre to centre; but it has for some time been the opinion of engineers that the common widths are too narrow for the desirable amount of stability and clearance.

"Deck" bridges are always employed when the requisite headroom beneath can be obtained without much additional embanking. They are more easily braced than "Through" bridges, are less exposed to the wind, are less liable to damage from passing trains, and require less masonry. The American engineers use very deep trusses for spans over 75 feet, but are careful to supply ample lateral bracing above and below. The West Shore Railway specifications require 20 feet clear headroom for all pin-connected through bridges, and these are consequently at least 23 feet deep.

The pin-connected bridges on the "West Shore" Railway are of the forms most commonly used in America, and called the Pratt and Whipple bridges.

The single intersection bridge is generally called the Pratt bridge, and the double intersection one, the Whipple; but these and other names are often loosely used for both arrangements.

In the Pratt and Whipple bridges the vertical members are all struts and the diagonals all ties. The struts and upper chord are formed of wrought-iron columns and latticed channel-irons, and the diagonals and lower chord of round rods and flat eye-bars. Pin-connections are used at all the joints, and the posts are sometimes riveted to the upper chord.

In American pin-connected through bridges the floors are generally arranged thus: riveted beams are hung from the pins at the lower panel points; wrought-iron rolled beams, 12 to 15 inches deep, usually two under each rail, are laid upon these cross-beams; and the track is spiked upon cross-ties resting on the I beams. The cross-ties are continuous for both tracks; where possible are usually white oak or yellow pine timbers, 8 inches square in cross-section, and are spaced 16 inches apart from centre to centre. The ties are notched ½ inch upon the I beams, and on the West Shore Railway fang-bolts fasten the timber to the iron. The bridges have no closer floor than that formed by the cross-ties.

The West Shore Railway specifications state the bridges of the railway shall be proportioned to carry the following loads:—

(1.) The weight of the iron in the structure.

(2.) A floor weighing 400 lbs. per lineal foot of track, to consist of the rails, ties and guard timbers only.

(3.) A moving load for each track, consisting of two "consolidation" engines coupled, followed by a train weighing 2240 lbs. per lineal foot.

Different bridge companies have different methods for reducing the "live" and "dead" loads to an equivalent dead load. Some engineers take out the stresses due to the sum of the dead and live loads, and add to those, to allow for dynamic effect, 25 per cent. for spans of 30 feet, 15 per cent. for spans of 50 feet, and 10 per cent. for spans of about 75 feet, the addition diminishing to 0 at spans of 100 feet. A much greater load per lineal foot is always provided for in short spans than in long ones for the following reasons:—first, the speed of trains is not usually slackened when short spans are approached, and the application of the load to them is therefore almost of the nature of a blow; and second, on short spans the whole bridge is frequently covered by locomotives, while larger spans can only be covered in ordinary working by adding to one or two locomotives a portion of the lighter train, and therefore they never have so high an average load per lineal foot as the smaller spans.

On some railways the maximum load assumed is 6,000 lbs. per lineal foot of single track, for 10-feet spans diminishing to 2,500 lbs. for 500-feet spans.

To provide for wind-strains and vibrations, the top lateral bracing in "deck" bridges, and the lower lateral bracing in "through" bridges, on the West Shore Railway, are designed to resist a lateral force of 450 lbs. per foot of span: of this 300 lbs. is treated as a moving load. The bottom lateral bracing in deck bridges, and the top lateral bracing in through bridges, are designed to resist 150 lbs. per foot of span.

All parts of the structures are specified to be so proportioned that the maximum loads shall in no case cause a greater tension than the following:—

	Lbs. per Square Inch.
On lateral bracing	12,000
„ solid rolled beams used as cross-floor beams or stringers	8,000
„ bottom chords and main diagonals	10,000
„ counter-rods and long verticals	8,000
„ bottom flange of riveted cross-girders, net section . .	8,000
„ „ „ „ longitudinal plate-girders over 20 feet long, net section	8,000
„ bottom flange of riveted longitudinal plate-girders under 20 feet long, net section	7,000
„ floor beam hangers and other members subject to sudden loading	6,000

Compression-members are so proportioned that the maximum loads shall in no case cause a greater stress than that determined by the following formulas:—

$$p = \frac{8{,}000}{1 + \frac{l^2}{40{,}000r^2}}$$ for square end compression-members.

$$p = \frac{8{,}000}{1 + \frac{l^2}{30{,}000r^2}}$$ for compression-members with one pin and one square end.

$$p = \frac{8{,}000}{1 + \frac{l^2}{20{,}000r^2}}$$ for compression-members with pin bearings.

p = the allowed compressive stress per square inch of cross-section.

l = the length of the compression-member in inches.

r = the least radius of gyration of the section in inches.

The lateral struts are proportioned by the above formulas to resist the resultant force due to an assumed initial strain of 10,000 lbs. per square inch upon all the rods attaching to them produced by adjusting the bridge.

In beams and girders compression is limited as follows:—

	Lbs. per Sq. Inch.
In rolled beams used as cross-floor beams or stringers . . .	10,000
„ riveted plate girders used as cross-floor beams, gross section	6,000
„ „ longitudinal plate girders over 20 feet long . . .	6,000
„ „ „ „ „ under „ „ . . .	5,000

Members subjected to alternate stresses of tension and compression are proportioned to resist each of them. The forces, however, are assumed to be increased by an amount equal to eight-tenths of the smaller stress.

In calculating stresses, the length of span is taken as the distance from centre to centre of pins for trusses, and between the centres of bearing-plates for all beams and girders. Where members are subjected to a bending stress from local loadings, in addition to the stress due to their positions as members of the structure, they are specified to be proportioned to resist the combined stresses.

The rivets and bolts in the structures are so spaced that the shearing-stress per square inch shall not exceed 6,000 lbs., nor the pressure upon the bearing surface exceed 12,000 lbs. per square inch of the projected semi-intrados, or diameter by thickness of piece, of the rivet or bolt-hole.

The specifications contain the following stipulations regarding riveting:—

(1.) The rivets will generally be $\frac{3}{4}$-inch or $\frac{7}{8}$-inch in diameter.

(2.) The pitch, in all classes of work, shall never exceed 6 inches, nor sixteen times the thickness of the thinnest outside plate, nor be less than three diameters of the rivet.

(3.) The distance between the edge of any piece and the centre of a rivet-hole must never be less than $1\frac{1}{4}$ inch, except for bars less than $2\frac{1}{2}$ inches wide; when practicable, it shall be at least two diameters of the rivets.

(4.) In punching plates, the diameter of the die shall in no case exceed the diameter of the punch by more than $\frac{1}{16}$ inch.

(5.) Whenever possible, the rivets must be machine-driven.

(6.) All joints in riveted work, whether in tension- or compression-members, must be fully spliced, as no reliance will be placed upon abutting joints. The ends, however, must be dressed straight and true.

Pins are so proportioned that the shearing-stress on them shall not exceed 7,500 lbs. per square inch, nor the crushing stress per square inch of projected semi-intrados be greater than 12,000 lbs., nor the stress due to bending greater than 15,000 lbs. per square inch when the centres of bearings of the stressed members are considered as the points of application of the bending forces. The diameter of the pins is not less than two-thirds of the largest cross dimension of any member attached to them, and they must be turned smooth and fit within $\frac{1}{50}$ inch.

Plate-girders are proportioned upon the supposition that the "bending or chord" stresses are resisted entirely by the upper and lower flange, and the shearing stresses resisted entirely by the web-plate.

The compression-flanges of beams and girders are stayed against transverse crippling when their length is more than thirty times their width, and the unsupported width of any plate subjected to compression never exceeds thirty times its thickness. The iron in the web-plates is proportioned to have a shearing stress not greater than 4,000 lbs. per square inch, and no web-plate is less than $\frac{1}{4}$-inch in thickness. No rods of less section than $\frac{3}{4}$ square inch are allowed for use as lateral or diagonal members. When the height of the trusses of "through" bridges is over 25 feet, overhead diagonal bracing is used. In all deck-bridges diagonal bracing is provided at each panel. In double-track bridges the diagonal bracing is proportioned to resist the unequal loading of the trusses.

All bed-plates are of such dimensions that the greatest pressure on the masonry does not exceed 250 lbs. per square inch, and they vary from $\frac{3}{4}$ inch to 2 inches in thickness.

Bridges of more than 50-feet span have at one end nests of turned friction rollers, formed of wrought iron, and running between planed surfaces. The rollers are not less than 2 inches in diameter, and are so proportioned that the pressure per lineal inch of roller does not exceed the square root of the diameter in inches, multiplied by 500 lbs. ($500\sqrt{d}$). Bridges less than 50 feet in span are secured at one end to the masonry; the other end is left free to move by sliding on planed surfaces. A variation in temperature to the extent of 150° is provided for throughout.

All bridges are given a camber, by making the panel length of the top chord longer than that of the bottom chord, in the proportion of $\frac{1}{8}$ inch to every 10 feet.

The specification for iron used on the West Shore Railway is similar to the one originally drawn up for the Cincinnati Southern Railroad. The same specification was adopted for the New York elevated railways, and is as follows:—

(1.) All iron must be tough, highly fibrous, and uniform in texture. In its manufacture no old or scrap-iron is to be used. Test-pieces, selected at random by the engineer from any iron designed to be used for this company, shall show a minimum ultimate tensile strength of 50,000 lbs. per square inch of original sectional area before fracture, and an elastic limit of not less than 25,000 lbs. per square inch of same sectional area, with an elongation before fracture of the test specimens of not less than 15 per cent., measured upon a tested length of 8 inches. Also a reduction of area at the point of fracture of 20 to 25 per cent.

(2.) All iron for tension-members must bend cold for about 90° to a curve whose diameter is not over twice the thickness of the piece, without cracking. At least one sample in three must bend 180° to this curve without cracking. When nicked on one side, and bent by a blow from a sledge, the fracture must be nearly all fibrous, showing but few crystalline specks.

(3.) Specimens from "angle," "plate," and "shaped" iron must stand bending cold through 90°, and to a curve whose diameter is not over three times the thickness of the piece without cracking. When nicked or bent its fracture must be mostly fibrous.

(4.) All rivets and connecting-pins must be made of the best double-refined iron, having an ultimate tensile strength of 60,000 lbs. per square inch, and be capable of being bent double when cold, so that the sides shall be in contact without showing any crack or breaking of iron fibre.

(5.) The cast iron must be of the best quality of soft grey iron, capable of sustaining a weight of 550 lbs. suspended at the centre of a bar 1 inch square $4\frac{1}{2}$ feet between supports. All manufacture must be full to dimensions, true, smooth, and sound.

(6.) All facilities for inspection of iron and workmanship shall be furnished by the contractor. He shall furnish, without charge, such specimens (prepared) of the several kinds of iron to be used as may be required to determine their character.

Near West Point, on the Hudson River, the longest double-track truss-bridge yet built has been erected to span a narrow bay of the Hudson. For several months during the early part of 1882 attempts were made to make a bank across this bay with materials from rock excavations on both sides. About 4,000 cubic yards of rock had been deposited, when it became evident that on account of the steep slopes the material was going out into the river nearly as fast as it was deposited. To obviate this, a wooden crib was built to fit the slope of the rock, and loaded with about 2,000 tons of stones. The crib was about 200 feet long, and 50 feet wide in the centre, with a depth in front of 40 feet, and, on the inside, next the bank, of 10 feet. It was anchored to the rock by 1½-inch anchor chains, fastened to 1½-inch ring-bolts, extending 18 inches into the solid rock. There were in all five anchor-chains, about 150 feet long. Without any warning this crib slid into the bed of the river, breaking the chains and lodging at the bottom, nearly 500 feet out, in about 150 feet depth of water. After this catastrophe it was decided to build a double-tract iron bridge 290 feet span from centre to centre of the end pins.

The use of steel for the bridge was contemplated, but the impossibility of getting prompt delivery of this material prevented its adoption. It was impossible to put up false-work that could be depended upon, and the erection was therefore somewhat of a problem. Two plans were considered: first, to erect the bridge on floats, but this was abandoned on account of there being a tide of 4½ feet, and because the base of rail was only about 6 feet above high tide. The second plan was to erect the bridge on one side of the permanent location, and then to slide it into place. This second method necessitated cutting into the solid rock of the mountain for a depth of about 40 feet at each end, but was finally adopted as the only safe plan.

The bridge was erected 25 feet to one side of the final site, and when completed, with the exception of the floor-system, was moved into its permanent place in six hours by means of hydraulic jacks. The total weight of iron in the finished span is about 519 tons. The bridge was designed and executed by the Central Bridge Works Company, of Buffalo, New York.

The railway crosses Rondout Creek at an elevation of 150 feet above water in order to admit the passage of large schooners. The total length of the crossing is 1,244 feet, and it is covered by a trestle-work viaduct 531 feet long, and three two-truss through-spans; one of 163 feet, one of 241 feet, and one of 264 feet span. The spans in the viaduct are six of 50 feet and one, next the large girders, of 60 feet, measured between facing columns. The girders in the viaduct are lattice girders 6 feet deep. The shortest long span has trusses 32 feet deep; the other two long spans have trusses 45 feet deep. The weight of iron in the whole structure is about 2,000 tons, and the total cost of the bridge was about $350,000.

The erection was begun on the 8th of November, 1882, and the first train crossed on the 8th of May, 1883. The ironwork was designed and constructed by the Phœnix Bridge Co. in accordance with the specifications of the West Shore Railway, and the compression members in the large trusses and the pier columns are of the pattern known as the Phœnix column. The erection of this great structure involved much care and labour. No falsework was used in erecting the viaduct. Each pier consists practically of four iron columns resting on masonry, and firmly held in position by struts and ties. After the foundations had been completed the columns were raised in place by means of a "gin pole," resting first on the masonry, and afterwards secured by iron bands to the columns as they were built up. As soon as the first span was bridged a traveller on the top assisted in raising the rest of the iron.

Much more difficult was the work of erecting the three great spans. In their case it was necessary to build a trestle-work sufficient to bear the weight of the iron and resist the wind, which sometimes sweeps with great violence through the gorge.

In this structure all bolts were $\frac{3}{4}$ inch in diameter; all diagonal transverse braces were 3 inches by 8 inches; all horizontal transverse braces 4 inches by 8 inches, and all longitudinal braces 3 inches by 8 inches.

The trestle-work was supported upon piles; the height from the top of the piles to the caps was 142 feet. Four hundred and sixteen piles, and 300,000 feet board measure of timber were used.

After the falsework had been erected the trusses were easily put together. The total cost of erection was about $1\frac{1}{2}$ cent per lb. This cost was from $\frac{1}{3}$ to $\frac{1}{2}$ cent per lb. greater than it would have been in summer. The weather during the erection of the bridge was unusually inclement, and the ropes, staging, &c., were covered with ice for much of the time.

A very interesting and novel lift-bridge (Plate 4) is employed to carry the West Shore Railway over the Oswego Canal at Syracuse. It was necessary to keep the formation low in passing through the city, and an ordinary overhead bridge could not therefore be used. Railway swing-bridges are common in America, and are consequently fairly cheap and of reliable design; but the angle of crossing in this case is so acute, and the width necessary for a two-track bridge so great, that a swing-bridge would have been extremely cumbersome.

Lift-bridges are of two kinds; first, that in which the whole structure, except four corner columns, is moved up and down; and second, that in which the floor is hung from stationary girders placed above the necessary clearance, and the floor only is moved. Highway bridges of both kinds are common, but only the second kind had yet been used for railways. The new bridge, however, is of the first kind, and the complete girder, floor and track are lifted through 10½ feet. The weight of the bridge is about 146 tons, and there was much interest and a little anxiety concerning the probable movements of this great mass until the work was finished.

The bridge is provided with counter-weights suspended by wire ropes.

The general width of the Oswego Canal waterway through the city of Syracuse is 60 feet; but at the point of crossing an encroachment of 20 feet was allowed, making the clear square span, including the tow-path, 52 feet. The angle of crossing is 50° from a right angle, and the span on the skew 81 feet 5½ inches.

The bridge abutments are walls of "first-class" masonry, 8 feet thick and 14 feet high, extending from a bed of shale about 3 feet below the bottom of the canal to the level of the tow-path. Four piers of the same height and 4 feet square are provided for the stays which brace the columns. The columns and stays are anchored by bolts 1½ inch in diameter built 8 feet into the masonry.

The distance between the centres of the columns is 96 feet, and the bridge is a "two-truss through," 94 feet long and 23 feet high, with chords 16 inches wide spaced 29 feet apart from centre to centre horizontally.

The rails are screw-spiked to oak sleepers 8 inches square in cross-section; these are spaced 16 inches apart from centre to centre, and are secured by fang-bolts to the flanges of the track-stringers. The track-stringers are rolled I beams, 12 inches deep, resting on angle-iron brackets riveted to the webs of plate cross-floor beams supported on the lower chords of the trusses at the panel points, which are 9 feet 3⅜ inches apart.

The floor is stiffened laterally by 1¼-inch rods connected with the ends of the floor beams and adjusted by sleeve nuts.

The counterweights are rectangular boiler-plate boxes 9 feet long by 4 feet square, closed at the bottom by a heavy casting and filled with pig-iron. Each counter-weight hangs from two steel wire ropes 1¾ inch in diameter furnished by the Roebling Co., New York. The ropes pass over cast-iron pulley-wheels of 7½ feet effective diameter and 3 inches wide, with grooves 2 inches wide and 3 inches deep. The connections between the ropes and the 3-inch square bolt passing through the centre of each counter-weight, and the two bolts 2½ inches in diameter, attached to each corner of the bridge, are simple and ingenious. The ropes are threaded through wrought-iron conical thimbles, the ends of the rope enlarged by folding back the strands, the enlarged ends drawn back into the thimbles, and the vacant spaces "run in" with "Babbits" metal well compacted by hammering.

The four counterweights weigh 140 tons and balance the bridge within about 6 tons. The small lifting power required is provided by a steam-engine with two 8-inch by 8-inch cylinders coupled at right angles, and placed with the boiler on top of the trusses.

Two lines of shafts 3 inches in diameter fixed on the top chords of the trusses turn four phosphor-bronze nuts fixed in collars to brackets on the end-posts of the trusses.

Through the nuts pass four double-threaded screws 3½ inches in diameter, with square cut threads of 2 inches pitch. The screws hang from the tops of the fixed columns and support the bridge when it is raised. One revolution of the engine gives the nuts half a turn, and lifts the bridge 1 inch. The screws are freely suspended at their upper ends, and when the bridge is lowered it adjusts itself to its seat, and the screws, raised from their bearings by a slight extra turning of the nuts, are supported free from stress.

On two of the end-posts of the bridge are guides connected with the columns. Each counterweight also has a grooved guide united to its column. The bridge can be raised in thirty seconds, but the usual time is thirty-five to forty seconds. The steam-pressure used is about 60 lbs. per square inch.

Near the bridge just described the railway crosses an important street, and the city authorities required that the railway company should lower a bridge carrying the street over the adjacent canal so that a clear view of the railway crossing could be obtained. The old bridge, one of "bowstring" form with a cast-iron arch and wrought-iron tension members, has been taken down, and a bridge with a lifting floor has been erected in its place.

The angle of crossing is 56½° from a right angle, and the span on the skew 83 feet. The floor was first built of riveted beams 2 feet deep, but after erection it was found necessary to stiffen it by adding two light trusses between the side-walks and carriage-way. The latter is 25 feet wide, and the side-walks each 6 feet wide.

The weight of the floor is about 60 tons, and it is fully balanced by semi-cylindrical troughs, filled with pig-iron and suspended by eighteen wire ropes of $\frac{3}{4}$ inch diameter, passing over the same number of cast-iron pulley-wheels, 2 feet in diameter, placed on the upper chords of the overhead stationary trusses. These trusses are two double Warren girders 104 feet long and 18 feet high, placed 26 feet apart on four columns 14 feet high. The floor is attached to four vertical channel-irons, to which four cast-iron racks 10 feet long and 8 inches wide, with teeth of 3 inches pitch, are bolted. The racks gear with four wheels 3 feet in diameter fixed on two shafts supported on the lower cords of the trusses. These shafts carry two toothed wheels 2 feet in diameter, which are turned by two horizontal racks fixed in the same line to the through piston-rod of a cylinder of 3 feet internal diameter and 6 feet length of stroke. The lift of the bridge, 9 feet, is thus effected by a single stroke of the piston, which is moved by water from the city mains.

The cylinder is in no essential point different from an ordinary steam-cylinder, and the water is controlled by a common "D" slide-valve. The water company's engineer was afraid that the comparatively quick cut-off by this valve would cause bursting shocks in the supply-pipes; but the bridge has been successfully worked without any indication of shock. However, to allay all fears, a spring relief-valve opening at 50 lbs. pressure per square inch, and an air-chamber 6 feet long and of 1 foot internal diameter, have been fitted to the pipes.

The supply-pipe is 6 inches in diameter and seems too large, as the bridge is raised in thirty-five seconds with a screw-down gate-valve controlling the supply opened only 1½ inch.

The ordinary maximum pressure in the city mains at the canal level is 30 lbs. per square inch. The water is discharged from the cylinder into a sewer 33 feet below, and the discharge-pipe has been carefully arranged in the hope that it will keep full and balance, by the column of water contained, the atmospheric pressure on the exhaust side of the piston.

In conclusion the Author would quote the following paragraph from a recent report upon the West Shore Railway by an engineer of great experience and high standing.

"This enterprise realizes the ideal road desired by railway managers. It has everything considered essential to the perfect road; nothing can be added. It has low grades, light curves, perfect track, and engines and cars of the most approved pattern. Everything is new; there is no old rolling-stock to be used up. Under these conditions (which heretofore have never existed on any road) the operating expenses will be remarkably low, lower than any road has ever attained. It will form the standard of comparison by which other roads are judged. Its annual reports will be read with great attention by railway managers and those interested in the transportation problem."

The Paper is accompanied by several sketches and tracings, from which Plates 3 and 4 have been prepared.

(Students' Paper, No. 179.)

"Emery Wheels and Emery-Wheel Machinery."[1]

By WALTER OSMOND ROOPER, Stud. Inst. C.E.

EMERY is a mixture of corundum and oxide of iron; corundum itself is alumina, with a little silica. Sapphire and ruby are corundum in its purest form, slightly tinged with iron oxide. In a less pure state, corundum is found in many places, and is then valuable only for commercial purposes.

The emery beds of most importance are those situated in Turkey and Asia Minor; considerable quantities of emery are found near Smyrna and Ephesus. The material takes its name from Cape Emeri, a promontory in the Isle of Naxos, where there are considerable deposits. In 1871 large beds of corundum were discovered in North Carolina; their existence having been betokened by the presence of numerous waterworn pebbles in the beds of the adjacent rivers. These beds have been worked commercially, and the corundum extracted, crushed, and used for grinding and polishing. Emery is obtained from surface workings, and the presence of the emery is generally indicated by the dark red colour of the ground above it. The prospectors strike steel rods sharply into the ground where they expect the existence of emery, and by examining the points of the rods can easily detect its presence. The mineral is broken up into lumps weighing about 1 cwt. each. If hard to break, a fire is lighted round the refractory lump, and on cooling blows are resorted to again. The lumps are then carried to the coast by railway, or on mule-back, and the material reaches England chiefly as ballast: thus freight is an insignificant item in the cost. The Turkish and Greek governments generally let the right to raise and dispose of the emery to English capitalists.

The specific gravity of emery ranges from 3·75 to 4·28, and the colour from dark grey to black; there is an impure variety of corundum blue grey to brown in colour. Much of the commercial emery is artificially coloured, and is often mixed to a small extent with ground iron slag; some buyers insist upon the adulteration.

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[1] This communication was read and discussed at a meeting of the Students, on the 25th of April, 1884, and has been awarded a Miller Prize.

Analysis of emery shows that it is composed of from 60 to 80 per cent. of alumina, 8 to 33 per cent. of iron oxide, together with a small quantity of lime water and silica.

Taking sapphire as a standard at 100, corundum from the Carolina mine has an abrasive power of from 90 to 97, and the best Naxos emery from 40 to 57 only. This would make it appear as if corundum were far superior to emery for grinding purposes; however, this is not the case on account of the peculiarity of the fracture of the latter. Every grain of emery breaks with a rough conchoidal surface, and presents numerous sharp points, be the lump large or small; even flour-emery exhibits this markedly when examined under the microscope. Corundum on the other hand, although breaking into pieces with many sharp points, exhibits also plane and curved surfaces. It is owing to this that corundum is not more extensively employed in the manufacture of grinding-wheels. Thus emery will cut its way into the material operated on, while corundum will grind and tear, and only perform the work in consequence of its being of a harder nature.

The next point to consider is the preparation of emery on its arrival in this country. It is first treated in the emery-mills; stone crushers reduce the large lumps to pieces about the size of a hen's egg, and these in turn are reduced in a stamping-mill, or are ground in a horizontal revolving mill with cast-iron circular grinding-plates. Naturally the life of these plates is not long, and frequent renewals are necessary. The fragments of emery are then passed between chilled cast-iron rolls, and ground or crushed again, then sifted through sieves. During these operations a portion of the emery is separated from the lumps in the form of dust, which collects on the top of beams and other places. This emery dust is all carefully preserved, and under the name of flour-emery is useful for polishing. Levigation is another process employed for separating the coarse grains from the fine emery. Six cylinders, about 3 feet high, are ranged in a row, and are in communication with one another at the top by means of metal pipes. Water mixed with emery from the rolls is introduced into the first vessel. The heavy grains sink, while the lighter ones remaining in suspension pass over into the next vessel. The process is repeated throughout the range; the last vessel containing the finest emery. There are now in this country a fair number of emery-mills, but in America the trade is of greater magnitude and more importance. Emery-crushers generally manufacture emery-cloth as a branch of their business, as also the flour-emery. The chief use of flour-emery is for making knife-polish, of which enormous quantities are sold. By far the largest demand for grain, or crushed-emery, is for the manufacture of emery grinding-wheels, which of late years have become familiar objects in most engineer's shops. Emery-wheel makers in this country do not as a rule crush emery. During the last ten years many classes of emery wheels have been introduced.

The emery wheels best known in England now are the "silicate" or "grey" wheel, the "vulcanite" or "black" wheel, the "shellac" or "red" wheel, the "union" wheel, and the "tanite" wheel.

The silicate wheel was invented by Mr. Frederick Ransome, Assoc. Inst. C.E., who has perfected the method of manufacture. He employed silicate of soda as the base of the wheel; emery wheels were made for some time by the Ransome Patent Stone Company, in conjunction with their artificial stone and grindstone business. The process consists first in the preparation of the silicate of soda. Beach flints, mostly brought over from France, and used in preference to English flints on account of their being associated with less chalk, are placed in a steam-jacketed boiler; caustic soda is added, and steam turned on. After some hours the dissolution of the flints is complete, very little solid residue being left. The liquid is then allowed to settle and run into a tank, where it is evaporated down to a consistency much like that of treacle, but as clear as sherry. This will remain liquid if properly made. Emery of the required degree of fineness is united with this silicate of soda in a mill, and a thorough mixing obtained; this "pug" is then pressed into moulds by hand-pressure. The disk produced would naturally harden by itself in time, but in order to save time it is baked, and is then complete and ready for use.

The manufacture of black or vulcanite emery wheels requires heavy and costly machinery. The base is india-rubber, oxidized oil, or other similar material. This is intimately mixed with the emery in steam-heated rolls (horizontal hollow cylinders); afterwards calendered to exact dimensions, and squeezed into moulds in a powerful hydraulic press. The disks are then cured in the same manner as vulcanized india-rubber goods; after which they are ready for use.

What is known as the red wheel is made with shellac as a base, and although the process is now considered old fashioned, excellent wheels for some classes of work are the result. Shellac is heated in an iron pot, the emery, which has been previously heated, is introduced, and the whole is well mixed by hand with iron spoons.

The pug is then put into moulds and squeezed in a hydraulic press; afterwards the disks are baked at a low temperature.

The tanite and union emery wheels are made in America; the methods employed in their manufacture are kept secret, and jealously guarded. In the tanite wheel a solution of leather, and in the union wheels oxychloride of magnesium, are the binding materials. The tanite wheel has the reputation of being the best made in America.

Emery wheels have been described as circular files which never grow dull. As previously mentioned the emery grains throughout the wheel retain their cutting power; and if these grains are cemented together with a proper binding material, an emery wheel can be worked until 90 per cent. of its original weight has been worn off. A file is useless before 5 per cent. of its weight has been used up, and when being worked is driven by manual labour at a speed of 90 feet per minute. An emery wheel penetrates without difficulty into the hardest metals, some wheels even cutting chilled iron or hardened steel more easily than soft iron, and it is driven by steam at a peripheral velocity of about 1 mile per minute. In using emery-wheels successfully it must be borne in mind that as the wheel wears away the revolutions per minute must be increased, so that as nearly as possible the same surface speed may be maintained upon the periphery of the wheel. The economy of emery wheels in the operation of fettling or trimming castings is very marked, and this relates not only to economy in tools, but also in the saving of time. American foundries, and many English ones, are well supplied with emery fettling machines.

An emery wheel keeps its size and shape far longer than a grindstone, and very little practice is required to use it successfully. It is a common thing to see a grindstone, 24 inches to 18 inches in diameter and 18 inches wide, slowly rotating in a trough, while men push and shove their tools against it to make any impression. It has been stated by an eminent firm of engineers in London that an emery disk as a tool grinder lasts out six 4-feet grindstones, and only requires trueing once a month, against the weekly trueing up of a stone. They calculate the cost of keeping an emery-wheel tool grinder in order as one-fifth of that formerly expended upon the maintenance of grindstones, and the time occupied in grinding tools one-half of that formerly required. In grinding tools, if very large quantities of metal have to be removed, it is best to grind them quickly, and to ignore their temper, rehardening them afterwards; but if only slight amounts, then the grinding should be gentle, and their temper will not suffer. Carpenters' planes can be ground by an emery wheel with ease after a little practice.

In this country many persons have failed in the use of the emery wheel owing to insufficient knowledge. Experienced men, who will take many precautions in fixing a lathe or planing-machine, will erect an emery machine anyhow, and then allow it to be used by any one. A very simple machine is sufficient, but it must be rigid, on a good foundation, having long bearings protected from dust, and must be so steady when in use as to be absolutely without tremor or vibration. A wheel should be mounted true and between two washers, screwed up moderately tight, never by driving the wheel on to a square spindle with a mallet, and tightening up the nut with a long-handled spanner. Care should be taken to run the wheel so that the nut tends to tighten itself upon the spindle. Attention should be given to keep the emery wheel true. In large factories one man should have charge of all the emery wheels, and should make his rounds at intervals to true the emery wheels with a diamond-tool. This tool consists of a small black diamond set in the end of a ½-inch steel bar, about 8 inches long fastened on a handle. In trueing, the rest is fixed close to the wheel, and on a level with its centre. The diamond-tool is then held firmly on the rest, and the point is gradually worked backwards and forwards until the projecting parts are removed, and the wheel runs true. The difference in working with a true wheel and one running out of truth can hardly be imagined by those unaccustomed to emery-wheel grinding. In the latter case grinding is a disagreeable duty, and the jar and vibration render the task of holding on the work very irksome. It is not by heavy pressure that the most and quickest grinding will be effected; the article merely requires holding up well and constantly against the wheel. Intelligent users soon get accustomed to feel the bite.

The most economical speed for emery wheels is about 4,000 to 5,000 lineal feet of circumference per minute. Some wheels are driven faster than this, and with good results; but owing to the jar which occurs if the wheel runs out of truth in the slightest degree, and to the necessity of such rigid foundations, a speed of 5,000 feet is seldom exceeded with success. There is, however, no doubt that the faster a wheel is run the more is the work accomplished.

Too much pressure on certain classes of emery wheels induces heat; and the melting of the india-rubber, or other substance used as the cementing medium, produces a glaze upon the cutting surface. By changing the metal being ground, as for example copper after iron, this glaze may generally be removed. In the case of wheels made with silicate of soda, or oxychloride of magnesium as the cementing medium, pressure only raises the temperature of the work, and does not injuriously affect the wheel. Experiments have been made with a view of ascertaining the utmost speed at which an emery wheel would run without bursting, but all the wheels tried have stood the utmost test applied. On the rare occasions when wheels have burst, the cause has invariably been traced to unsteady bearings, improper adjustment of the wheel in the first instance, or an article being jammed between the wheel and the rest. But it is well known that far fewer accidents arise from the bursting of emery wheels than from the bursting of ordinary grindstones.

It must not be forgotten that as different shapes and varieties of steel tools are necessary for different operations upon a lathe or planing machine, so different classes of emery wheels are required for different purposes, and it is the best economy thus to apply them. One class of wheel should be kept for one class of work whenever practicable. Wheels can be made hard or soft, rough or smooth, slow cutting or quick cutting; as a rule, fast-cutting wheels have a rough surface, and slow-cutting a fine one. As in the case of an ordinary grindstone, the faster a wheel cuts the faster it will wear away. A wheel can be made so hard that after six months' usage no wear to speak of will have taken place; but the amount of work such a wheel would have turned out would be infinitesimal, compared with that which a softer wheel would have done, and the money and time lost in working the hard wheel would have purchased many soft ones.

In the machinery for mounting emery wheels, first comes the tool-grinder, a common object in large works, and invaluable for putting a fine cutting edge on tools, grinding them to template, or reducing them quickly. The emery wheel, generally of a fine grade, is mounted in a small cast-iron trough cast on a pedestal, extra long bearings are provided, and a cap which will completely cover the upper half of the wheel, excepting 3 or 4 inches where the rest is fixed a little above the centre of the wheel-level. A continuous water-supply is provided by means of a small pump, and a tank within the pedestal. The cap serves to keep the water from flying off the wheel whilst rotating.

The emery surfacing-machine has only been introduced lately.

It is probably an English idea, although modifications introduced from America. A cast-iron box, with a to in a vertical direction, has long bearings across it. In the spindle upon which the emery wheel is fixed. Th top of the box has a hole cast or ground in it, just large admit a small portion of the periphery of the emer protrude, so that the wheel is otherwise entirely bo small fan revolves at the bottom of the box, and coll dust and cuttings which follow the wheel, thus helpi the surface of the table clean. By means of a screw the table can be raised or lowered, and a greater or less wheel exposed. The article to be ground is passed bac forwards on the table top over the protruding part of As the table is exactly true, only the projecting parts of can be touched by the emery wheel, and thus flat s rapidly attained. A steel key, which would take a fit hour to complete, can be ground true by an emery w minutes. The top of the table should consist of a gro plate. Slide-bars for locomotive engines, after b hardened, are generally ground true in a special mac bar is cramped down in a horizontal trough, which m wards and forwards under the emery wheel. This with runs at right angles to and above the trough. The b supported by horns from the machine standards. wheel spindle, in addition to its rotatory motion, has a c so that during the forward and return journey of th every portion of it comes into contact with the emery- trough has a reciprocating motion like a planing-mac Dust from the wheel and slide-bar gets into the bearin wears them down, so that great attention is necessar Adams, M. Inst. C.E., of the London and South Weste at Nine Elms, put in some metallined bearings answered admirably, and have run without oil or four years. During that time his machine has sur thousand and forty slide-bars, and the wheel, which w in diameter at starting, has only worn down to 22 i lateral travel of the machine is 3½ inches, and it m once to about every thirty revolutions of the spindle.

Crank-pins after case-hardening are trued up at the works by means of emery wheels. A pin is placed in lathe, and driven at a low speed. A 14-inch by 2- wheel is fitted up solidly on the slide-rest, and is drive drum on a counter-shaft, the small pulley on the c spindle being provided with flanges to keep the driving strap on. Case-hardened and chilled rolls are trued up in the same manner, and this method of removing the superfluous metal from such hard material has proved very successful, not the least advantage being that no stoppages for fettling tools are necessary.

In railway and repairing shops, where new ends are brazed upon old boiler-tubes, emery wheels are useful for removing the scurf from the joint, and reducing it to the required diameter.

Pulley-grinding is another common use to which emery wheels are put. Cast-iron pulleys are often ground just so that their hard surface is removed, and a tool can enter without being injured immediately. Wrought-iron pulleys are almost invariably ground in a large surfacing lathe, the emery wheel running on the rest, and being driven from an overhead drum. This is no doubt an economical way of getting up wrought-iron pulleys, and through its adoption lighter iron can be used for the rim of the pulley than if it were going to be turned true with a tool.

The long knife emery-wheel sharpener is for grinding and sharpening the knives used by paper-makers, tobacco-cutters, and book-binders. The knife is fixed in a slide-rest, which has a self-acting rectilinear motion in front of the emery wheel (generally here a large sized one), which runs in long bearings at the back of the machine, with its spindle parallel to the knife. As the knife is only ground by the periphery of the wheel, a certain concavity dependent upon the size of the wheel is produced upon the edge of the knife. This concavity is so small that it is ordinarily disregarded. In an American machine recently introduced this defect is remedied by using an emery cup-wheel, fitted on to the end of the shaft, which runs at right angles to the knife, and thus performs the grinding with its face, and produces a flat surface.

The ordinary fettling machine for foundry- and yard-work consists of a heavy vertical cast-iron box 2 feet by 3 feet by 2½ feet high, with two long bearings on the top, and a steel 1¾-inch spindle running in them. The driving-pulley is often keyed on the spindle between the bearings, and two heavy emery wheels overhang on each side. Sometimes one emery wheel is fixed in the middle of the machine between the bearings and pulley outside. Such a machine is invaluable for dressing heavy castings, and is also useful in girder-work for taking off the arisses left by the saw, and for cutting the bevel of the T iron stiffeners. A smith takes from three and a half to four minutes to cut the bevel of a 3-inch by 4-inch T, and a man can grind off the bevel with an emery wheel in from one to one and a half minute, saving at the same time the wages of a striker, and the cost of repairing sets. Smaller fettling-machines are made for lighter work, and these are generally bolted down to the bench, or on to a stand made for them. Agricultural and other engineers who have a variety of small articles to get up find emery wheels very useful.

Coming to lighter work, the emery-tape machine is a handy tool. It consists of an upright frame 5 feet high, with a stout spindle running in one long bearing 12 inches from the ground. At one end of this spindle are fixed the fast-and-loose pulleys for driving, and at the other end the emery-band driving-pulley, 2 feet or more in diameter and flanged, of a width corresponding to the width of the largest emery-band it may be desired to use. Guide-pulleys to hold the band up to its work are provided on the side of the machine, and an adjustable flanged pulley for tightening the band at the top of the frame. The band consists of an endless tape prepared exactly in the same way as emery cloth. It is made in various widths. The driving-pulley is generally urged so that the emery-band has a surface speed of 3,000 feet per minute. Implements of irregular forms can be readily polished on this machine, and the band can be worked either wet or dry. Emery-wheel saw-sharpeners are now familiar objects in most saw-mills, and their economy over the old process of sharpening by hand is obvious.

In conclusion, it may be well to mention that, in order to ensure success in the use of emery wheels, plenty of power must be provided. No experiments, it is believed, have been made with the object of ascertaining the exact power required to drive any particular size or class of emery wheel; but it may be assumed that a 14-inch emery wheel will take as much power to drive it properly as a large lathe.

Small as is the use of emery wheels at the present time, it will not be rash to predict that in a few years every engineer's shop in this country will be well supplied with emery-wheel machinery.

(*Students' Paper, No. 176.*)

"Constructional Ironwork for Buildings."[1]

By RICHARD MORELAND (*tertius*), Stud. Inst. C.E.

THE use of iron for building purposes has made rapid strides in the last eighty years. Before this time, the floors of warehouses, factories, &c., had been supported on timbers, which were often rather irregularly placed together, but they served their purpose, as they had not to carry any great load. When cast-iron columns and girders were advocated as a substitute for timber, it was thought that, if the strongest timbers were not able to support the required load, such a material as cast-iron would not answer, as it was then looked upon as a very brittle and unreliable substance.

One of the first instances of the successful employment of cast-iron was at the beginning of this century, for the columns and girders of a cotton-mill. These were designed by Messrs. Boulton and Watt; the girders were of a ⊥ section, the bottom flange being 3¼ inches wide, the depth 13½ inches, and the thickness of metal 1¼ inch (Plate 5, Fig. 19). They would stand, by calculation, a weight of 8·32 tons in the centre. Although increased security was gained by these improvements in cast-iron girders, their use was nevertheless attended with danger, and in three or four instances, about the year 1844, there were some alarming accidents in consequence of the failure of cast-iron beams.

Since that time wrought-iron girders and rolled-iron joists have been extensively used.

The employment of iron for buildings and roofs has so many advantages, that the demand for structures entirely or partially of this material continually increases; and the rapid development of blast-furnaces, rolling-mills, and foundries in Great Britain has rendered available for building purposes numerous new forms and qualities of iron. At present, in almost every warehouse, factory, mill, or building used for commercial purposes, the best and most economical method is to use iron columns and girders. In warehouses where no very large unsupported space is required, and where the material and goods are continually being shifted, the most economical practice is to place tiers of columns, extending right to the top of the building, at about 10 or 12 feet apart, and between them light angle-iron plate-girders, or rolled-iron joists; the floor-joists rest on these in the manner shown in Fig. 3, and the boards are nailed on to them. As there are no girders in the direction in which the wood floor-joists run, the columns should be attached to the walls by means of the rods or light rolled-iron joists, thus rendering the structure rigid.

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[1] This communication was read and discussed at a meeting of the Students, on the 8th of February, 1884, and has been awarded a Miller Prize.

Most of the warehouses in Wood Street are constructed in this manner, the goods being piled round the columns, and a passage being kept on the outside. Taking one of these warehouses, for instance, in London Wall, Figs. 1 and 2 show the general arrangement of the constructional ironwork. The whole area is about 1,950 square feet. The columns, which are of cast iron, are placed at about 12 feet apart from centre to centre; the following are the various diameters for their respective floors:—

Inches.		Inch.							
12	diameter	1½	metal	from	sub-basement		to	basement.	
11	,,	1½	,,	,,	basement		to	ground floor.	
10	,,	1⅜	,,	,,	ground	floor	to	1st	,,
9	,,	1⅜	,,	,,	1st	,,	,,	2nd	,,
8	,,	1¼	,,	,,	2nd	,,	,,	3rd	,,
7	,,	1	,,	,,	3rd	,,	,,	4th	,,
6	,,	⅞	,,	,,	4th	,,	,,	roof level.	

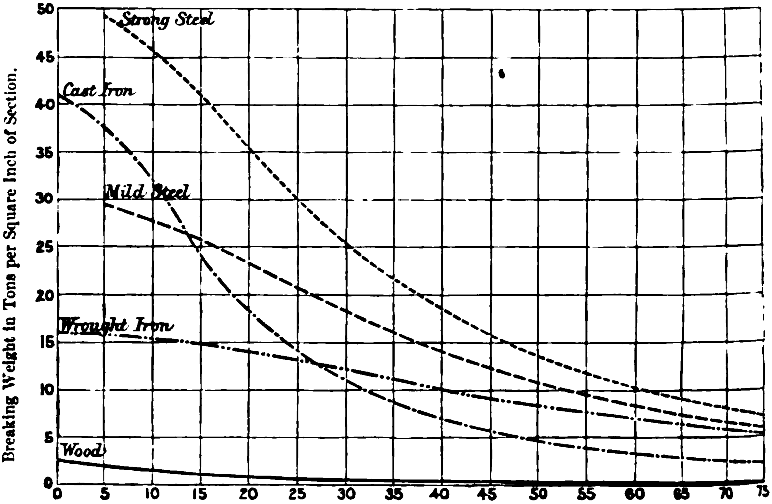
The lengths varying from 10 feet 6 inches to 12 feet, and the joints are all faced.

Separate bases, 3 feet 6 inches square, are used for each tier of columns. One advantage of placing the columns as above is that the weight is more equally distributed over the area, the load sustained by the concrete in this case being 126 tons on each base. The section of the girders and lintels is throughout the same (Figs. 3 and 4); the girders are from 11 to 12 feet long, and the lintels from 10 to 13 feet. As this building was required to be constructed in the shortest possible time, this method was eminently suitable, the whole of the ironwork having been erected, including roof principals composed of wrought-iron ribs, in three months, the total weight of material used being 95 tons, at an average price of £10 per ton. These floor girders will carry a safe equally-distributed load of 19 tons, the weight per square foot being taken at 2½ cwt. throughout each floor; they are also subjected to severe shocks from falling weights.

The weight per square foot for ordinary dwelling-houses is generally 1¼ cwt.; for public buildings, 1½ to 2 cwt., and for warehouses, factories, mills, breweries, &c., from 2½ to 5 cwt. In a cooperage recently erected, where wood is stacked as closely as possible from floor to floor over the whole surface, the ironwork was calculated to sustain 4 cwt. per square foot. In factories where machines are placed on the floors, and where there is great vibration from them, as in printing-machines, the floors should be made of good thick concrete, to deaden the shocks; the floor shown in Fig. 10 is an example.

On investigating the compressive strains on columns, struts, &c., it will be found necessary to provide not only against the absolute crushing of the material, but more especially against the flexure and buckling, to resist which a greater amount of material is generally required than theory might seem to indicate. A very short pillar, such as a cube, will bear a weight almost sufficient to crush it to powder; while a thin plate of ductile material, such as a penny, will support almost any load. A long thin pillar, on the other hand, will fail by flexure before it is crushed; and if broken, the fracture will resemble that due to a transverse strain. Hence the results of compressive strain must be divided into flexure and crushing.

When the length of a pillar is not less than its diameter, and not more than four or five times its diameter, fracture is caused by the two ends of the prism forming cones or pyramids, which split the sides and throw them out. In cast iron, the angle of fracture is such that the height of the wedge is somewhat less than one and a half time its diameter. In considering the kind of material best adapted for pillars for supporting ordinary loads such as frequently occur in warehouses, factories, &c., it will be found that, if the length of the pillar were not to exceed four or five times its diameter, it would only be necessary to calculate the strength of the pillar by multiplying its transverse area in square units by the crushing weight per square inch of that particular material. As pillars are rarely so short as this, their strength must be calculated when they fail, not by crushing only but also by flexure. If the line of thrust always coincided with the axis of the pillar, then the ratio of diameter to length need not be considered, and the rule quoted before would suffice; but this is practically impossible, for a slight error in the line of thrust produces a corresponding tendency in the pillar to bend. This tendency to bend renders it necessary to construct long pillars, not merely with sufficient material to resist crushing, but also with such additional material as may preserve them from yielding by flexure. In masonry, the desired effect is produced by the bulk of material, which ensures the line of thrust always lying within a safe distance of the limits of the structure. In hollow columns, the same result is obtained by making them of sufficient diameter; that is, by removing the material to a considerable distance from the line of thrust, which, although it may deviate slightly from the axis of the pillar yet will not pass outside the circumference.



Ratio of Length to Diameter.

The accompanying Fig. shows the breaking weights of various materials in the form of pillars, taking into consideration the ratio of length to diameter. Mr. Hodgkinson found that the external part of a casting, whether hollow or solid, was always harder than that near the centre, the hardness increasing with the thinness. Removing the skin of a casting reduces its strength to resist compression. In large castings the difference of strength between the external and internal parts is much less than in small ones.

In hollow columns, the thickness of the metal is frequently less on one side than on the other; but this does not produce so great a diminution in strength as might be expected, for the thinner part of the casting is much the harder. In practice neither the excess nor want of thickness should exceed 25 per cent. of the average thickness. A good proportion is to make the metal one-twelfth the diameter of the pillar, but this is generally exceeded as small diameters are usually preferred, 12 inches being the outside limit for columns inside a building. Founders have no difficulty in making castings of uniform thickness. In some hollow columns recently tested for a building in Oxford Street, which were 10 inches in diameter and the metal 2¼ inches thick, the thickness was found to be perfectly uniform. Although a short pillar of cylindrical section of cast-iron will support a much greater load than a similar one of wrought-iron, yet a solid wrought-iron pillar more than twenty-six diameters in length will bear a greater weight than a similar one of cast-iron, because its coefficient of elasticity is considerably higher than that of cast-iron.

If the strength of long pillars of similar section remains constant, their transverse area will vary as their length, and their weight as the square of their length. By long pillars are understood those, of cast-iron, whose length is thirty diameters and upwards, and pillars which fail by flexure, their breaking-weight being far short of that necessary to crush the material when in short pieces. The weight which will deflect a long pillar is very near the breaking weight; a long wrought-iron pillar, for instance, may be expected to fail on the concave side, and a similar one of cast-iron will probably fail on the convex side, on account of their respective resistances to tension and compression. Long pillars with flat ends, and firmly bedded, are three times stronger than pillars with rounded ends, so that all columns, stanchions, &c., should be firmly bedded on their base, and all joints should be planed and well fixed, so that the line of thrust may coincide with the line of axis; pillars irregularly fixed lose from two-thirds to four-fifths of their strength.

The crushing-strength of cast-iron is about 38 tons per square inch; the strength of mixtures, however, is about 42 tons. Repeated meltings seem to have the effect of increasing the resistance to crushing of cast-iron within a given limit. The crushing-strength of wrought-iron varies with the hardness of the iron; but ordinary wrought-iron is crushed, or bulged, at a pressure of about 16 to 20 tons per square inch.

The practical limit of compressive elasticity of average steel is about 21 tons per square inch. Cast-iron pillars supporting loads free from vibration such as grain, &c., should not be loaded with more than one-sixth of their calculated breaking-weight. In factories, where strong vibrations from machinery occur, the working load should not exceed one-eighth; and if the pillar be liable to transverse strains or severe shocks, like those on the ground-floor of warehouses where loaded wagons or heavy bales are apt to strike against them, the load should not exceed one-tenth of the breaking-weight.

It is also necessary to take into consideration the foundations on which the pillars rest; for if these yield unequally one pillar may sustain much more than its proper share of load. For practical purposes a safe working load of one-tenth the breaking-weight is generally taken for cast-iron.

The working strain of wrought-iron pillars, when subject to shocks like the jib of a crane, should not exceed one-sixth of the breaking-weight; but with quiescent loads one-fourth is a safe margin.

The resistance to crushing of steel is so high that even 15 tons per square inch is perhaps a safe compressive working strain, when the material is not allowed to deflect; but when in the form of a solid pillar the strength seems to be about one and three-quarter time that of wrought iron.

Comparing the relative strengths of three forms now much used for building purposes, and taking the strength of a hollow cylindrical pillar as 100, that of an H-shaped pillar will be 75, and of a +-shaped pillar, 44. For solid pillars of equal length and area, the proportional relative strengths are:—a long solid round pillar 100, a square one 93, and a triangular one 110. For practical purposes the round pillar is the most economical, since the shape of the triangle will generally prohibit its use.

The relative compressive strengths of various materials in long columns is:—cast iron 1,000, wrought iron 1,745; cast steel 2,518, and oak 109.

There are various other forms of pillars of struts; for calculation, the least diameter is to be measured in the direction in which the pillar is most flexible.

Tables 1 to 3 show the differences, &c., in strength of various sections of equal length and area, of cast-iron pillars. Table 4 shows the variation in different forms of wrought-iron pillars.

Although cast-iron girders were soon displaced by wrought-iron, yet there are numerous buildings in which the loads are still supported by girders of the former. In a brewery erected in 1874, all the cellars are arched in with 9-inch brick arches, the weight of which is taken by cast-iron girders, supported on columns of similar material (Fig. 6). The whole structure is fastened together with wrought-iron tie-rods 1¼ inch in diameter, and covered in with concrete 6 inches thick on the crown of the arch, and paving laid on this. These cast-iron girders are of

TABLE 1. CAST-IRON PILLARS, with an EQUAL LENGTH of 15 FEET, and an EQUAL AREA of 20 SQUARE INCHES.

Form and Dimensions in Inches of Section.	Diameters in Length.	Breaking-Load in Tons per Square Inch of Section.	Breaking-Load.	Percentage of Strength above that of Solid Cylinder.	Working Load $\frac{1}{10}$th of Breaking Load.	Square of Radius of Gyration of Section, r^2.
			Tons.			
Solid cylinder, 5·1 diameter	36	8·00	160	..	16·0	1·50
Solid square, 4·47 on side	40	8·25	165	3	16·5	1·60
Unequal H stanchion and unequal channel, 10 × 6 × 1 . . .	30	9·00	180	12	18·0	1·75
Cross of equal arms; angle-iron of equal webs; equal tee-iron, 10 × 10 × 1 . . .	25	11·70	234	46	23·5	2·50
Equal H stanchion and equal channel, 7¼ × 7¼ × 1 . .	25	13·50	270	68	27·0	3·00
Hollow square column, 6 × 6 × 1 . . .	30	16·50	330	106	33·0	4·25
Hollow cylindrical column, 7½ diameter, 1 metal	24	17·80	356	123	35·5	5·00

TABLE 2. CAST-IRON PILLARS, with an EQUAL LENGTH of 17 FEET, and an EQUAL AREA of 38 SQUARE INCHES.

Form and Dimensions in Inches of Section.	Diameters in Length.	Breaking-Load in Tons per Square Inch of Section.	Breaking-Load.	Percentage of Strength above that of Solid Cylinder.	Working Load $\frac{1}{10}$th of Breaking Load.	Square of Radius of Gyration of Section, r^2.
			Tons.			
Solid cylinder, 7 diameter	29	11·25	427	..	42·0	3·00
Solid square, 6⅛ on side	34	11·40	433	1·4	43·0	3·10
Unequal H stanchion and unequal channel, 12 × 8 × 1½ . . .	25	12·50	475	11·0	47·0	3·50
Cross of equal arms; angle-iron of equal webs; equal tee-iron, 13½ × 13½ × 1½ . .	20	15·50	589	31·0	59·0	5·00
Equal H stanchion and equal channel, 10 × 10 × 1⅜ . .	20	17·50	665	55·0	66·0	6·25
Hollow square column, 8 × 8 × 1½ . . .	25	19·30	733	71·0	73·0	7·75
Hollow cylindrical column, 10 diameter, 1⅜ metal	20	20·75	788	84·0	72·0	9·00

TABLE 3. CAST-IRON PILLARS, with an EQUAL LENGTH of 2
EQUAL AREA of SECTION of 81 SQUARE INCHES.

Form and Dimensions in Inches of Section.	Diameters in Length.	Breaking-Load in Tons per Square Inch of Section.	Breaking-Load.	Percentage of Strength above that of Solid Cylinder.
			Tons.	
Solid cylinder, 10·2 diameter . . .	24	14·6	1,182	..
Solid square, 9 on side	26	15·5	1,255	6·0
Unequal H stanchion and unequal channel, 20 × 12 × 2. . .	20	15·0	1,215	2·0
Cross of equal arms: angle-iron of equal webs; and equal tee-iron, 21 × 21 × 2 .	16	18·0	1,458	23·0
Equal H stanchion and equal channel, 15 × 15 × 2 . .	16	21·5	1,741	47·5
Hollow square column, 12 × 12 × 2 . .	20	24·0	1,944	64·0
Hollow cylindrical column 15 diameter, 2 metal	16	25 0	2,025	71·0

TABLE 4. EXPERIMENTS MADE by MR. DAVIES on WROUGHT-

Form and Dimensions in Inches of Section.	l Length.	d Least Diameter.	Area.	t Thickness of Metal.	Ratio $\frac{l}{d}$.	Ratio $\frac{d}{t}$
			Sq. Ins.	Inch.		
Angle-iron, 3 × 3 × $\frac{5}{16}$.	60	2·25	1·78	0·3125	26·7	7·20
Tee-iron, 3 × 3 × $\frac{3}{8}$.	60	3·00	2·1	0·3750	20·0	8·00
Channel-iron, 3 × $1\frac{3}{4}$ × $\frac{3}{8}$.	60	1·75	2·15	0·3750	34·2	4·60
Cruciform, $2\frac{1}{2}$ × 3 × $\frac{3}{8}$.	60	2·125	1·88	0 3750	28·2	5·66

These all failed by bending in the centre.

the section shown in Fig. 7, the depth being 15 inches; the bottom flange is 12 inches wide by 1½ inch, and the top flange 3 inches by 1½ inch; the thickness of the web is 1¼ inch at the top, tapering to 1½ inch at the bottom. The span is 15 feet. They are well suited for their work, as a skewback can be formed in brick, and the concrete, &c., gives them lateral stiffness.

A wrought-iron beam, as shown in Fig. 8, with angles 3 inches by 3 inches by ⅜ inch, and a web 15 inches by ⅜ inch, will carry the same safe load as this, namely 14 tons equally distributed, the weight of the cast-iron girder being 133 lbs. per lineal foot, and that of the wrought-iron only 44 lbs. per lineal foot. The cost of the former is about 7*s.* per cwt., and of the latter about 11*s.* Taking these two considerations into account, it is found that the cost of the cast-iron is about twice that of the wrought-iron.

Cast-iron girders are still used successfully for lintels, as in the warehouses at London Wall. (Fig. 4.)

According to Messrs. Fairbairn and Hodgkinson, the area of the bottom flange of girders should be six times that of the top, when the load is on the bottom flange; this is about the ratio of the one shown in Fig. 7. When the load is on the top, the bottom flange should only have three times the area of the top flange, as in Fig. 4.

Cast-iron has the advantage that it can be made to almost any form or shape. A girder, where the thickest amount of metal is required for the flange in the centre, can be made of parabolic shape, which is the proper theoretical form. Wrought iron, on the other hand, cannot be made in this form without a great deal of extra expense.

On account of imperfections and flaws in casting, and unequal contraction of the metal, cast-iron cannot be depended upon for beams which have any great weight on them likely to produce shocks, and it has therefore been greatly superseded by wrought-iron, on account of its superiority against every kind of strain except quiescent compressive strain.

Of various wrought-iron beams, comparing weight for weight the strength of the box-girder (Fig. 12) and the plate-girder (Figs. 13, 14), it will be found that the former is the stronger in the proportion of 100 to 93. The difference in the resisting-strengths of these two beams arises from the superior form of the box-girder, which has a larger exterior sectional area, and is consequently stiffer, and better calculated to resist lateral strain. In this respect the plate-girder generally fails, before its other powers of resisting tensile and compressive strains can be brought fully into play. When, however, the plate-girder is used for supporting the floors, or the arches of fireproof buildings, it has the requisite lateral stiffness imparted to it, and its strength is nearly equal to that of the box-girder.

In the case of bressummers, or places where the girder has no lateral support outside itself, then the box girder is the best, not only on account of its lateral stiffness, but also on account of the flange being better supported. Wide brick walls are often built on the top of these (Fig. 12).

Bressummer girders having no other side supports, should have sufficient breadth to give them lateral strength; the cost of box-girder is about £13 per ton, fixed.

The plate-girder, although not equal in strength, is in some respects superior to the box-girder, being more easily constructed, less expensive and more durable, besides admitting of easier access for painting.

Another method has been extensively adopted for increasing the lateral stiffness of the plate-beam, by bolting pieces of timber on both sides of the plate; an example is shown in Fig. 18, as used in a building in Paternoster Square.

Comparing the results of experiments with a plate-girder, this form of beam has been found to be weak. The strain cannot come on each member at the same time, on account of the indifferent manner in which the beam is put together.

The wooden plates, generally bolted to the webs of girders, for supporting the ends of the wooden floor joists (Figs. 3 and 9), almost attain this end, namely, that of imparting lateral stiffness.

The most economical form of plate-girder is that shown in Fig. 14, where no great flange area is required. It is made simply of two angle-irons riveted to the web both top and bottom; these angles can be obtained in sections up to 6 inches by 6 inches, unequal sides being also rolled, such as 6 inches by 4 inches and 4 inches by 3 inches; these latter sizes are admirably suited for shallow girders, where the part of the angle near the centre of the girder is not required.

This form of section possesses an advantage over a girder with top and bottom plates, as Fig. 13. Take, for instance, the area of the bottom flange of each girder. In the case of Fig. 14 the gross sectional area is 10·35 square inches, and on deducting the area of the rivet holes, namely, 1·25 square inch, the net area is 9·1 square inches. In Fig. 13 the gross sectional area is 11·8 square inches; but the area of the rivet holes is more than twice the amount, or 2·7 square inches; this gives the same effective area as the girder composed simply of angle-iron, so that there is a loss of 2·7 square inches against 1·25 square inch by the material punched or drilled out for the rivet holes. On estimating the weight of these two sections, it will be found that the angle-iron girder weighs 80 lbs. per lineal foot, whereas the plate-flange girder weighs 98 lbs. per lineal foot; assuming the length of each to be 20 feet, and taking the cost of each at 11*s.* per cwt. and 11*s.* 6*d.* per cwt. respectively, the angle-iron girder only costs £7 17*s.* in comparison with the plate-flange girder amounting to £10 1*s.*, or, which is equivalent, to about 25 per cent. less.

The flanges of wrought-iron girders are generally of equal or nearly equal area, top and bottom; for the deduction of rivet-holes in the tension flange is compensated by the higher unit strain in the net area, which is effective for tensile strain; and as the gross area becomes effective in the top flange for compressive strain.

A useful form of girder is the rolled iron joist (Fig. 17). Where the depth does not exceed 12 inches; these joists are much cheaper than riveted girders, and they are now largely used in buildings. Above this depth considerable strains seem to be set up during the rolling by the contraction of the webs taking place before that of the flanges. For larger loads the joists are put together, as in Fig. 15, to form a composite girder. These girders cost about as much for labour as a riveted girder, and in some cases the iron being very brittle, great difficulty is experienced in punching the flanges, the holes frequently having to be drilled. In one particular instance, where the holes were punched in the flanges of a 12 inches by 5 inches rolled-iron joist, the metal surrounding the hole burst out in almost every place.

Angle-iron girders seem to be preferable to joists, the former giving more bearing surface for wooden plates.

Fig. 20 shows a cast-iron beam with tie-rods attached; this is considered to be rather a dangerous form of construction, as there is great difficulty in getting the proper tension in the rods, to suit the extension of the cast-iron.

A girder when riveted up should resemble as nearly as possible a beam constructed of one piece of iron; if this is not so, it must bend to undue extent. Riveting is best effected by machine, worked either by steam or hydraulic power, as the heated rivet is compressed, and completely fills the hole. If the rivet has been hammered by hand, on knocking the head off, it can easily be driven out, but, on the other hand, if riveted by machine, it is generally necessary to drill it out.

If a riveted joint connects plates in tension, the aggregate shearing area of the rivets on each side of the joint line, multiplied by the safe shearing unit strain of the rivets, should equal the working-strain transmitted through the plates. In practice, the rivet-area is generally one-tenth greater, in order to compensate for any inequality in the distribution of the strain among the rivets. When a joint connects compression-plates whose ends do not butt closely together, the thrust is transmitted through the covers, and tends to shear the rivets across exactly in the same way as when a tensile strain is applied, and the same rule holds good here. If, however, the compression-plates have their ends planed and brought up in close contact, a short cover, with one or two transverse rows of rivets on each side of the joint line, will suffice; this close contact, however, is impracticable, on account of the plates drawing away from one another while the flange is being riveted.

Where possible the plates should be made in one length, as this does away with the need of covers. In large girders, although the cost of the long plates may be higher, yet this is defrayed by the saving in material and time in putting in cover-plates. The strength of a riveted joint, as far as the rivets are concerned, is proportional to the number of shears to which they are subjected; rivets in double shear being twice as strong as those in single shear, so that a single-shear must have twice as many rivets as a double-shear joint. Practically the shearing-strength of wrought iron is equal to its tensile strength.

Punching reduces the tensile strength of the iron to a greater extent than the aggregate area of the metal punched out, and in most cases the fibre of the iron can be seen to be injuriously affected. Drilling does not damage the metal round the hole, and is therefore preferred when the work will permit of the extra cost. Mr. Maynard inferred, from experiments, that drilled plates are 19 per cent. stronger than punched ones. This percentage will depend on the condition of the punching-machine, being obviated slightly by making a proper proportion between the punch and die, and also on the quality of the iron and the thickness of the plates, a hard brittle iron, such as that used for the Belgian rolled joists, suffering considerably more than the Staffordshire brands, and thick plates more than thin ones. Mr.

Maynard also concluded that rivets were 4 per cent. weaker in drilled than in punched holes, because the sharp edges of the drilled plates tend to shear the rivets cleaner than those in punched plates, and he inferred that the difference was about 15 per cent. in favour of the former.

Rivets contract when cooling, and draw the plates and angles together with such force that the friction is generally sufficient to prevent them from slipping over each other, so long as the strain lies within limits which are not exceeded in practice. The value of this friction is variable; in experiments made with ⅞-inch rivets through two and three thicknesses of plate it amounted to from 5·5 tons to 7·9 tons before the plates slipped. When this happens, the rivets are not subject to shearing-strain. Though the friction of riveted plates may be sufficient to transmit the ordinary working strain, without subjecting the rivets to shearing, it does not follow that the ultimate strength of a rivet-joint is increased by this friction.

Rivets should be of closer pitch in the top flange than in the bottom, to prevent buckling. Where the rivets are required to be flush for the purposes of bearing-surfaces, special plates should be provided with countersunk holes, so that the rivet may have a head, otherwise the part of the rivet which is flushed projects about ⅛-inch above the surface, which is objectionable, especially where girders take their support on stone templates in the wall.

The working-strain is that to which any material is subject in practice. As a general rule one-sixth of the breaking-strain may be taken as the safe working strain for cast-iron girders, subject to vibration; but when the load is stationary and free from vibration, such as water-tanks, &c., one-fourth of the breaking-strain is safe.

For wrought-iron girders, supporting warehouse floors, one-fourth of the breaking strain is about a mean working-strain.

The following Table shows the working- and breaking-strains of cast and wrought iron and steel :—

Material.	Nature of Strain.	Factor of Safety.	Working-Strain.	Breaking-Strain.
			Tons per Sq. In.	Tons per Sq. In.
Cast iron . . .	Compression . .	⅙	6 to 8	36 to 48
„ . . .	Tension . . .	⅙	1¼ „ 1½	7½ „ 9
Wrought iron . .	Compression . .	¼	4	16
„ . .	Tension . . .	¼	5	20 to 22
Steel	Compression . .	¼	12 to 15	48 „ 60
„	Tension . . .	¼	8	32

The great difficulty of obtaining two pieces of iron alike is one reason why an ample margin of safety should be allowed; this difficulty often occurs in the case of rolled plates or bars, the thickness varying a good deal in the length.

The durability of ironwork greatly depends on the efficiency with which it is painted. The importance of regular painting is often not sufficiently appreciated; and in many existing structures decay is actively at work. With wrought-iron girders, sustaining immense loads, the risk of ultimate disaster through this cause is considerable. Ironwork completely embedded in brickwork and masonry is preserved from rust by the lime; but it is advisable to paint it before being built in.

In the process of casting iron, the molten metal fuses the sand upon the surface of the mould, and produces on the casting a skin which has the appearance of a silicate. This skin gives a smooth hard surface to the iron, and as this would, if allowed to rust, soon be destroyed, it is advisable to protect it by paint. In the case of wrought iron, the elements of rust are present from the commencement, as the skin forms a scale which must fall off sooner or later. To prevent this oxidation, it is sometimes specified that the iron while hot shall be dipped in boiled oil; but the working on the iron afterwards is sure to leave some places exposed.

When iron is galvanized, the scale is removed by dipping it in dilute acid, and then washing it in pure water, before the zinc can adhere to the iron. If the trouble and expense were not so great, this would be the proper process of preparing iron for paint; but the same results may to some extent be obtained by allowing the iron to rust, and then scraping off the scales preparatory to painting.

According to the ordinary plan of painting iron, the outer skin is only preserved for a time, and will become loose and fall off in scales; it is then that the real iron is reached, and permanent protection may be effected; but before the second coat of paint is put on, the first should be scraped off. Two or three coats of paint should be given to ironwork; and the paint should be renewed according to circumstances in about three to five years.

In constructing a fireproof building, there are several important principles to be observed. The structure should be of incombustible materials, and in order to prevent the spread of fire, when it has broken out, every opening communicating with the outer atmosphere should be closed; and an isolated stone or iron staircase should be provided for each storey, well protected on each side by stone walls. Perhaps the best fireproof buildings are composed of brick or stone, with iron beams and columns, properly tied together with rods built into the walls, and brick arches for the floors. The arches are ordinarily filled up with a concrete of lime or sand; and are either covered with flags or tiles, bedded in mortar, or when a wooden floor is necessary, the boards are generally nailed on to wooden sleepers imbedded in the concrete (Figs. 6 and 10). This when properly constructed forms a floor perfectly impervious to fire, provided that the iron columns and beams and brick arches are made strong enough to resist the impact of the falling of heavy goods. To prevent the heat of fire from affecting the ironwork, it is advisable to encase it in cement concrete. Another method of fireproof flooring (Fig. 9) consists of main girders 18 inches deep and 7½ inches wide, consisting of two $\frac{5}{16}$-inch plates on the top and two ¼-inch ones on the bottom flange, the angle-iron being 2½ inches by 2½ inches by ½ inch throughout, and the span between the walls being 32 feet. These girders were placed at 5 feet 6 inches pitch along the whole length of the building, which was 136 feet long; and the concrete was supported by light T iron bars, 2½ inches deep, and 1¼ inch wide, weighing 2¼ lbs. per foot, at 9 inches pitch along the girders. The girders are very expensive to manufacture on account of the extra amount of labour entailed in having two plates instead of one in each flange; and the flanges being narrow makes the riveting by machine more troublesome: had angle-iron girders been used instead, as in Fig. 10, the mode of construction would have been an economical one. The concrete is 6 inches thick.

In a building now being erected at Kentish Town, angle-iron girders, 15 inches deep, 6 inches by 4 inches by ½ inch for the large ones, and 5 inches by 4 inches by ½ inch for the smaller ones, are placed at intervals of 12 feet 6 inches and 7 feet 6 inches, and the concrete, which is 14 inches thick in this case, is supported by rolled-iron joists of 7¼ inches by 3¾ inches for the large bays, and 6¼ inches by 2 inches for the small bays. This concrete is covered with a wooden floor, the boards of which are nailed on to wooden sleepers imbedded in the concrete.

In warehouses the means of excluding air is of vital importance in the case of fire, and the use of iron shutters to windows, and double iron doors, seems to meet this point. In all the warehouses in Wood Street, there are two iron doors at each exit on each floor, which are shown in Fig. 2, and a stone staircase is provided in each block of buildings. Had the floors in these warehouses been fireproof, it seems unlikely that the fire there in December, 1882, would have proved so disastrous.

All the examples of ironwork mentioned in this Paper have come under the Author's personal notice; and have been executed by the firm of Messrs. Richard Moreland & Son. The Paper is illustrated by several small scale drawings, from which the Fig. in the text and Plate 5 have been prepared.

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(*Paper No. 1994.*)

"The Basic, Open-Hearth, Steel Process."

By Thomas Gillott, M. Inst. C.E.

In the following account of the application of the basic, or Thomas-Gilchrist, dephosphorization process to the open hearth furnace, the Author has confined himself to his own personal experience, dating from May 1882, at the works of the Farnley Iron Company, near Leeds. The basic Bessemer process is now well known and established, and will probably be the most extensively adopted for the production of steel in large quantities for general use; but, as in the contemporary acid processes, the Siemens furnace retains advantages for the production of exceptional qualities, and has the further capability of dealing with raw materials which are altogether unsuited for the Bessemer (basic) process.

Pig iron most suitable for the basic Bessemer process should approximately contain, according to Messrs. Thomas and Gilchrist, "silicon, 0·5 to 1·8; phosphorus, 0·8 to 3; sulphur, under 0·3; manganese, not over 2·5 per cent.;"[1] and, in order to secure the necessary heat, the carbon would probably have to be 3·5 per cent.; but in the basic open-hearth process the proportion of heat-producing elements need not be high, and large quantities of wrought-iron scraps, which often contain 0·2 per cent. of phosphorus, and are altogether unsuited for the acid process, may be converted into steel of high quality and great purity.

After unsuccessful attempts to produce a high-class steel in the acid process by the use of hæmatite pig for the bath, and wrought-iron scraps, or specially puddled iron made from pigs in which phosphorus was comparatively high, the Author was instructed to apply to the open-hearth furnace the process which Messrs. Thomas and Gilchrist had at that time just succeeded with in the Bessemer converter. As no basic bricks, excepting such as were too costly for practical use, were suitable for constructing the furnace throughout, the Author consulted chemists as to materials which would be sufficiently refractory at a steel melting heat, and at the same time securely isolate the dolomitic lime basin of the furnace from the silica walls above; so as to prevent that fluxing of lime and silica, and consequent destruction of the furnace, which would result from these materials being in immediate contact. He was advised that magnesia on the lime, alumina above the magnesia, and silica brick-work carried up on the alumina, would be secure; so, after some trials, bricks were made from magnesite containing 98 per cent. of carbonate of magnesia, and others from bauxite of the approximate composition, silica, 15, alumina, 82, lime, 1, peroxide of iron, 2; and with these the furnace was built in accordance with Plate 6, Figs. 1 to 4.

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[1] Journal of the Society of Arts, vol. xxx. (1882), p. 648.

This furnace had been previously used in the acid process for 50- to 55-cwt. charges, and was altered to suit the basic process. At the commencement the Author recognised the necessity of providing for the removal of the slags in the early stages of the process, so that the banks of the furnace might be preserved; and any phosphorus transferred from the melted portion of the charge to the slags could be withdrawn, so as to reduce the amount of purification at the later stages, and allow the heat to be better employed by reducing the covering of slag on the metal; and this provision has proved very useful.

The building of the furnace was conducted as follows: the iron bottom-plates A and the bridge-plates B are similar to those used for the acid furnace; brackets bolted to the side-plates of the furnace support the brick-work by the plate C at the level of the top of the lime basin, and prevent settlement of the upper brick-work in case of any shrinking of the lime basin or fluxing in work. The metal tapping-hole and spout is at D, and two slag tap-holes are at higher levels E E on the opposite or charging side of the furnace. In other respects the furnace does not differ from an ordinary Siemens furnace. Bricks made from magnesian lime, and of the following approximate composition: lime, 58; magnesia, 24; silica, 8; alumina and oxide of iron, 10 parts; are burnt at a full white heat, crushed, mixed with hot tar in a mortar mill, and are taken immediately to the furnace and rammed with red-hot rammers over the parts G on the furnace bottom-plates, wooden plugs being inserted to form the tap-holes, the parts marked H being walled in the same bricks, and grouted with material similar to the ramming. The isolating courses I and K, of magnesia and bauxite bricks respectively, are laid on the top of the iron brackets C on the sides, and across the ends of the basin over the plates B; and the shape of the hearth is completed by lime ramming.

The use of hot rammers is necessary to coke the tar; otherwise the lime swells and falls to pieces in about twenty-four hours

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after mixing. The upper part of the furnace is then completed in silica-brick in the usual manner, the gas- and air-passages, regenerators, reversing valves, &c., being similar to those used in making steel in the (acid) Siemens-Martin process.

The building of the furnace having been completed and the furnace lighted, it will be sufficiently hot in about seventy-two hours for forming the bottom, and for this a full melting heat is necessary. Well burnt and freshly ground magnesian limestone is laid over the bottom and up the sides, with not more than 10 per cent. of ground fire-brick added, so as to just flux the lining sufficient for binding the layer forming the furnace-bottom. The wooden plugs for forming the tap-holes are burnt out, and the holes are stopped by dry lime only, or dry lime mixed with a small quantity of powdered coke. The gas- and air-ports should be so arranged as to throw the flame well down on the bottom, so that the heat may be sufficient to allow of the smallest possible admixture of silicious flux.

The furnace is then ready, and the operation is thus conducted: fresh and well-burnt lime (that used was from stone containing 98 per cent. of carbonate of lime), about one-twentieth to one-tenth of the weight of the pig iron, is laid upon the bottom of the furnace, the pig is then charged, and the wrought-iron or steel scraps are put on the top of the pig, all being cold. The whole of the charge if possible is placed in the furnace at the commencement; feeding afterwards is always avoided when the state of the furnace will admit. In three or four hours, the pig being melted, the charge will have sunk so as to enable some slag to be tapped off. The first slag is generally very fluid, and more or less so as the silicon in the pig is higher or lower; consequently the sides of the hearth are far better preserved by getting rid of the more silicious slag at an early stage, and before the full heat is reached. In three and a half to six hours, the time being dependent on the proportion of pig to scrap, the whole charge will be melted, more slag can be taken off if required, some lime added if the slag is very fluid, and ore with the lime if the proportion of pig to scrap is high. When boiling has ceased and after the bath of fluid metal has been well stirred, the metal may be sampled. The samples are taken from the furnace in a ladle, cast into an ingot about 3 inches in diameter by 2 or 2½ inches thick, and flattened, when sufficiently cool, under a steam hammer into a plate ¾ inch thick. This is quenched in cold water and then bent double, broken in two pieces and the larger portion broken again. When the metal is sufficiently pure these samples are so tough as to be scarcely, but nearly, broken through by being flattened close; the fracture is generally free from crystals, of good colour, and uniform. Crystals in specks are sometimes found, but when regular as regards size, and equi-angular to the naked eye as regards shape, the presence of crystals does not always indicate less purity than does their absence. Long streaks in the fracture, dark colour, with little toughness, are more certain indications of the presence of phosphorus. The sample being satisfactory, the slag may be removed; but at this period it is generally too pasty to run, therefore it is withdrawn by raking through the centre door which is placed 4½ inches lower than the two end doors. About 5 per cent. of hæmatite pig is then charged through the end doors on the bridges, which pig when melted causes the metal in the bath to boil more or less violently for about fifteen minutes. After this, fifteen to thirty minutes are allowed to elapse, and any excess of slag beyond what is required to cover the metal in the casting ladle is removed, when the metal in the bath is ready for adding spiegel or ferro-manganese, and tapping. The addition of hæmatite pig causes a thorough agitation of the bath far beyond what can be done by stirring with an iron bar; and every analysis the Author has had made proves a further removal of phosphorus by the addition. There is a drawback, however, to this on account of the occasional failure of parts of the basin during the after-boil, which causes a further boiling out of the bottom or sides of the basin in the neighbourhood of the failing place. Sometimes the tap-hole stopping commences to work out during the after-boil; and these accidents are generally due to some of the lime used for repairs, or stoppings, being imperfectly burnt; or it has been partially slaked during, or after, grinding. The Author has tried several times to secure the quality of steel aimed at without adding pig at the end of the purification, but the results have been uncertain and irregular. In only one instance has the Author obtained evidence of re-phosphorization on adding the ferro-manganese after adding hæmatite pig. In this case the sample that had been flattened, quenched, and broken, tested 0·104 per cent., and the sample from the finished steel showed 0·122 per cent. of phosphorus. In other cases where the two respective samples were tested for phosphorus, the reduction per cent. averaged 0·016, and varied from 0·008 to 0·025 less phosphorus than the trial samples contained. When quite satisfactory the trial samples show 0·06 to 0·07 per cent. of phosphorus.

The Author has always added the ferro-manganese in the furnace, as the reaction is somewhat too violent to permit of addition being made in the casting-ladle after the metal has been tapped; and a more uniform mixture is secured by adding in the furnace.

A small quantity of slag is usually left on the metal so as to cover the steel in the ladle and avoid chilling while pouring the ingots. Some of this slag is nearly always left on the furnace bottom, and is raked out at the door; and some also remains in the tapping-hole. If the slag be not removed immediately, the clearing of the tap-hole is a troublesome operation, and for this purpose a door opposite to the tap-hole is absolutely necessary.

After casting the metal and raking out the slag, the bottom and sides of the furnace-basin are repaired by magnesian lime, well burnt and finely ground, and any holes that may have been formed during working are carefully filled with the same kind of material. The gas is turned on to fix the lime by semi-fusion with the surface of the basin, the tap-hole is stopped, and the furnace is ready for another charge in from one hour to three hours after the cast is over. The slag adhering to the bottom of the furnace requires occasional removal by melting off with fluxes, or breaking away; otherwise the capacity of the furnace is reduced and the level of the metal unduly raised.

The quality of metal which the Farnley Iron Company desired to produce was that which possessed the maximum ductility when cold, together with the capability of sustaining, without failure or injury, the severe tests required by the most difficult flanging and welding when hot. In this respect, the results of the mechanical tests which will be submitted are a sufficient proof of success; the chemical features will be dealt with separately. But it may here be remarked that Farnley Best Yorkshire cold-blast pig iron has been the only phosphoric pig iron used; and being so low in silicon and sulphur, regular in quality, and more free from sand adhering to the pig than the cheaper hot-blast irons, a large portion of the successful results obtained must be credited to the principal raw material used.

Two typical casts will now be given, (229), one in which pig with a small admixture of wrought-iron scraps was worked with ore; and another (375), with less pig, more scrap, and no ore.

			Tons.	cwt.	qrs.	lbs.
No. 229.	Charge 6.30 A.M.	Farnley No. 3 pig	1	8	0	0
29th June, 1882.	,,	Steel scrap	0	8	2	0
	2.45 P.M.	Hæmatite pig, No. 3 . . .	0	2	0	0
	3.10 ,,	Spiegel, 18 per cent. Mn. .	0	1	1	0
	3.25 ,,	Ferro-Mn., 53 per cent. . .	0	0	1	5
			2	0	0	5

Commenced adding ore and lime at 10.30 A.M., 4½ cwt. Sommorostro ore, and 1¾ cwt. burnt lime; 2 cwt. of slag run off 2.40 P.M. Sample flattened, quenched, broke tough, without any crystals in fracture: 3½ cwt. of slag removed. Cast 3.28 P.M. One 15 inches square, and one 12 inches square, full ingots. 12-inch ingot hammered into a slab and rolled into a plate 8·10 × 5·4½ × ½ inch bare. Strips from plate heated red, quenched, bent cold, and closed to ⅜ inch between folds without crack. The plate, after annealing, was bent to 2 feet 9 inches diameter, welded longitudinally, and flanged at both ends without crack or failure.

[Tests and analysis are given afterwards.]

			Tons.	cwt.	qrs.	lbs.
No. 375.	Charge 6.30 A.M.	Farnley No. 3 pig . . .	0	17	0	0
	,,	Common wrought-iron scrap	0	6	0	0
	,,	Wrought turnings . . .	1	2	0	0
	1.20	Hæmatite, No. 3	0	2	0	0
	..	Ferro-Mn., 71 per cent. . .	0	0	2	19
			2	7	2	19

4 cwt. of slag taken off, and about 1½ cwt. left on metal. 1.10 P.M. Sample flattened, quenched, broke very tough; no crystals in fracture.

	Ton	cwt.	qrs.	lbs.
Cast 2.25 P.M., 1 ingot, 19 inches square . .	1	17	0	26
1 piece, 12 inches square . .	0	5	2	14

Hot and cold tests satisfactory. Phosphorus in finished steel = 0·056 per cent.

The design of the furnace is in several respects unsatisfactory for basic work. When lime is added during the melting, small quantities, carried by the draught on the faces of the silica bricks forming the ports, rapidly flux the gas- and air-passages, which causes a silicious liquid to trickle down across the furnace, and a further fluxing of the lime basin on the top of the bridge-plates B is occasioned by the droppings from the faces of the ports. The Author has not yet succeeded in making a lime brick that will withstand the current of gas and air, and the cooling down of the furnace after tapping. Under the combined effects, disintegration of the lime brick occurs, and any failure of the ports must be repaired at once—for which the furnace has to be cooled down. Usually the ports need repair after eighteen to twenty-one days' work; the side wall on the tapping side lasts about twice as long as the ports, and the crown of the furnace in the centre requires minor repairs at the same time as the side walls. Showers of minute sparks, with brown smoke at the chimney-top, are given off in the later stages of the process; and the regenerators require cleaning out after sixty or seventy casts. The slag tap-holes in the furnace used are too near the jambs between the charging doors, but from the arrangement of the furnace no other position could be found for them.

To remedy these inconveniences, the Author has designed another furnace (Plate 4, Figs. 5 to 9), in which the gas- and air-passages are carried up outside the hearth and in separate casings, so that they may be changed and the inlets repaired without cooling down the furnace. This arrangement of gas- and air-ports has been patented by Messrs. Hackney and Wailes. Three doors for charging and repairing, and two slag-spouts are provided on the opposite side to the tap-hole for the metal, and two sight-holes, L L, for inspecting the furnace while working. The other reference letters apply to the same parts as the furnace before described.

Owing to the very soft quality of steel produced, the heat required is nearly as great as Sheffield silica bricks are able to withstand; so long as the slag can be kept clear of the magnesia and bauxite bricks they have been satisfactory, and, but for their great cost, magnesia bricks would doubtless be much better than silica bricks for the faces of the ports. The waste is not excessive; taking the more recent work, without charging or crediting skulls and pit-scrap, the results are as under:—

CASTS 302 to 408 (inclusive).

Materials charged.

	Tons.	cwt.	qrs.	lbs.
Farnley pig iron	90	8	0	0
Hematite ,, ...	9	13	2	0
Iron and steel scrap	130	5	0	16
Ferro-Mn.	4	14	2	14
	235	1	1	2

The total weight, including spare metal, of ingots produced from the above is 219 tons 6 cwt. 3 qrs. 26 lbs., or about 93 per cent. of the raw materials. About 9 cwt. of raw dolomite per ton of ingots has been used for the repairs of the furnace, as deduced from the work done during one year.

The following mechanical tests are from casts of the numbers annexed; nearly all the plates have been for boiler-work requiring difficult flanging; excluding some three or four accidental casts, they are a fair average of the quality of steel.

Breaking-weight per square inch.	Elongation in inches.			Reduction of Area.	Description of Piece tested.
	10	8	2		
Tons.	Per cent.	Per cent.	Per cent.	Per cent.	
25·89	25·00	27·50	53·12	55·75	No. 229 Plate from ingot 12 inches square; size, 2·015 inches × 0·46 inch.
22·29	31·25	34·06	53·12	62·90	No. 242. Axle from 15 inches square ingot; billet turned to 1-inch diameter.
24·96	35·00	37·50	62·50	62·73	No. 245. Plate from 12 inches square ingot; size, 2·03 inches × 0·37 inch.
23·00	30·00	31·25	62·50	63·48	No. 246. Plate from 15 inches square ingot; size, 1·99 inch × 0·755 inch.
24·16	27·50	30·47	..	56·53	No. 289. Plate from 15 inches square ingot; size, 2·035 inches × 0·59 inch.
23·92	28·12	34·18	59·37	58·25	No. 304 Plate from 19 inches square ingot; size, 1·52 inch × 0·68 inch.
25·98	24·37	26·56	40·62	50·00	No. 305. Billet from axle from 19 inches square ingot, turned to 1 inch diameter.
26·68	23·75	25·00	43·75	46·64	No. 313. Plate from 19 inches square ingot; size tested, 1·765 inch × 0·69-inch.
26·68	26·25	28·47	53·12	49·77	No. 315. Plate from 2 feet 3 inches × 1 foot ingot; size tested, 1·52 inch × 0·705 inch.
24·07	28·75	31·25	50·00	48·93	No. 345. Plate from 19 inches square ingot; size tested, 2 inches × 0·565 inch.
24·76	27·99	30·62	53·12	55·49	Average.

All the test-pieces were parallel for a length of 11 inches, plates being machined on the edges only; a space of 10 inches long was in every case divided into 10 equal parts. The cases in which a length of 2 inches adjacent to the fracture showed a greater elongation per cent. than the contraction of area per cent., are explained by the concave shape of the ruptured edges, so that when the pieces were joined for measuring the elongation, the fractured edges were in contact at the sides only; and the true elongation is consequently less than the distance between the points, from which the percentages in the Table are calculated; this only occurs with rectangular sections.

The pig irons used were of the following composition:—

ANALYSIS by J. O. ARNOLD, F.C.S., IRON MAKER.

—	Farnley.	Hæmatite.
Iron	93·311 (by difference)	93·194
Graphite	3·250	3·798
Combined carbon . .	0·392	0·410
Silicon	1·245	2·285
Sulphur	0·013	0·004
Phosphorus . . .	0·601	0·058
Manganese . . .	1·188	0·199
Titanium	..	0·152
	100·000	100·000

The wrought-iron scraps used were not all of high quality, and would average not less than 0·2 per cent. of phosphorus; consequently charges 229 and 375, before given, would respectively contain 0·46 and 0·35 per cent. of phosphorus at the commencement, the samples taken from the casting-ladle in both cases showing that the phosphorus had been reduced to 0·067 and 0·056 per cent. respectively. Other phosphorus tests from casts for which the charges were similar to 375, the selection being made from a number, are as follow:—

Casts, No.	242	271	273	274	275	276	283
P. per cent.	0·065	0·078	0·090	0·097	0·092	0·086	0·097

Casts, No.	287	300	301	305	335	336	340	348	351
P. per cent.	0·076	0·083	0·082	0·050	0·083	0·076	0·064	0·079	0·053

Casts, No.	352	360	375	382	383	402
P. per cent.	0·060	0·071	0·056	0·055	0·056	0·062

All the foregoing phosphorus tests were from ladle-samples taken after tapping, except No. 305 which was drilled from a finished forging; some of those given are of casts where the phosphorus was expected to be somewhat high.

The Author is indebted to Mr. J. O. Arnold, F.C.S., for all the analyses of metal and slags, excepting that of the hæmatite pig.

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[1] The Journal of the Iron and Steel Institute, 1879, p. 158.

As in three samples of Farnley pig iron sent to other chemists, the phosphorus was respectively returned as 0·38, 0·317, and 0·29 per cent., and also as the percentage of phosphorus in the finished steel appears high compared with the published results of basic Bessemer work, the Author requested Mr. Arnold to furnish him with an account of the method by which the phosphorus was estimated, and his note is given in an Appendix to this communication.

The order of removal of phosphorus is evidently different from that which it has been shown to be in the Bessemer converter. In the latter process it has been proved that the phosphorus is not eliminated to any great extent until the carbon and silicon have almost disappeared; consequently the Author was scarcely prepared to find so small an amount as 1·974 per cent. of phosphoric acid in the final slags of charge No. 229. He therefore took a sample of slag from No. 301, which had been run off after the pig was fluid, but before the scraps were melted, and this sample when tested afforded 5·087 per cent. of phosphoric acid.

Although samples of metal have been taken when the whole charge was melted, and before boiling commenced, the phosphorus in the melted metal was always found to be much lower than the charge was known to contain originally, proving that the elimination of phosphorus in the basic open-hearth process commences in the early stages.

With respect to the removal of sulphur, the results were not so encouraging. The following Table gives three tests; (*a*) when the charge was all melted; (*b*) from the flattened test sample; and (*c*) from the finished steel. They were taken from casts in which scraps known to be high in sulphur were purposely tried.

No. of Cast.	Sulphur Per Cent.			Phosphorus Per Cent.		
	a	*b*	*c*	*a*	*b*	*c*
213	0·226	0·192	0·218	0·114	0·066	0·070
214	0·230	0·182	0·172	..	0·039	0·041
225	0·200	..	0·189	..	..	..
227	..	..	0·197	..	..	..

None of these charges were satisfactory under the hot tests.

The evolution of sparks and brown smoke has been before referred to, and as the Author in several instances was unable to balance the phosphorus in the charge with that contained in the slags added to the phosphorus left in the finished steel, he took samples of the deposits in the regenerators. One sample from the gas regenerator contained 4·741 per cent., and another sample from the air-regenerator gave 2·995 per cent. of phosphoric acid (P_2O_5), the other constituents being S_1O_2, Fe_2O_3, CaO, and MnO. These deposits appear to indicate that some of the phosphorus leaves the furnace otherwise than with the slags.

Three analyses of finished steel are furnished from charges that have been already given in detail: one sample (229) was taken by a sample-spoon from the casting-ladle, but the other two, Nos. 245 and 345, both similar charges, were drilled from the specimens tested in the machine, the tensile strengths, &c., of which are included in the Table of tests.

—	No. 229.	No. 245.	No. 345.
	Per cent.	Per cent.	Per cent.
Carbon . . .	0·240	0·140	0·140
Silicon . . .	trace	trace	0·004
Sulphur . .	0·060	0·037	0·074
Phosphorus .	0·067	0·056	0·050
Manganese .	0·526	0·191	0·598

A quantity of slags from pig and scrap charges similar to Nos. 245 and 345, all being taken from those run off before the final additions and ground up, gave on analysis S_1O_2 17·830, and P_2O_5 2·726 per cent.; and a complete analysis of slags tapped out of the furnace with the metal, and consequently taken after the additions of hæmatite pig and ferro-manganese is given below.

	Per cent.		Per cent.
P_2O_5 . . .	0·806	CaO	39·700
S_1O_2 . . .	13·640	MgO . . .	11·750
Fe_2O_3 . . .	1·370	MnO . . .	8·762
FeO	18·571	S	0·119
Al_2O_3 . . .	2·235	Alkalies and loss	3·047
			100·000

The Author has purposely omitted the question of cost, but it may be stated that the process is more expensive as regards furnace repairs, and greater attention is required in working, although the

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number of men employed at the furnace is nc required by the acid process. The preparation for repairs, namely, calcining and grinding dolomi limestone for adding, is not a part of the duties and involves greater expenses than those for corres the Siemens, or Siemens-Martin processes.

For the production of exceptionally soft steel and the utilization of much wrought-iron scrap an of phosphoric pig that in the present acid processe good steel, the basic open-hearth process offers pecu and will doubtless be further developed in the fut

The Paper is accompanied by several tracin Plate 6 has been prepared.

APPENDIX.

NOTES ON THE ESTIMATION OF PHOSPHORUS IN BASIC STEEL.

By J. O. Arnold, F.C.S.

The estimation of phosphorus in steels containing so minute a quantity as that usually present in basic steel requires considerable care and precaution to obtain accurate results. The method employed for the determination of the foregoing phosphorus returns is carried out in the following manner:—

Dissolve 5 grams of steel in nitric acid sp. gr. 1·2, evaporate the solution to dryness, and heat strongly to drive off the acid. Take up the dry mass of oxide of iron (which contains the phosphorus as phosphate) in strong hydrochloric acid, dilute the solution, and add sulphurous acid to reduce the iron from the ferric to the ferrous state; boil off the excess of sulphurous acid, and re-oxidize a small portion of the iron with bromine water; nearly neutralize with ammonia, add ammonium acetate, and boil. The red precipitate thus obtained contains the whole of the phosphorus; filter it off, dissolve in hydrochloric acid, evaporate gently to dryness, to get rid of free acid and to separate the last traces of silicon, and then take up in a little water; add ammonia in excess to throw down the iron, take up with strong nitric acid adding excess, boil to low bulk, add excess of ammonic molybdate solution, and again boil. A yellow precipitate of ammonia-phospho-molybdate is obtained, mixed, however, with free molybdic acid; this latter determines the precipitation of a small amount of phosphorus, which would otherwise remain in solution if the temperature were kept low enough to obtain only the yellow salt. Collect the precipitates on a filter, wash with nitric acid water till quite free from iron, dissolve in ammonia, add fifteen drops of magnesia mixture drop by drop, with constant stirring; allow the liquid to stand for a night, filter off the precipitated double phosphate of magnesia and ammonium, wash with ammonia water, dry, ignite strongly, and weigh; a snow-white salt is thus obtained, containing 27·97 per cent. of phosphorus, calculate the percentage on 5 grams of steel. The bulk of the liquid for the precipitation should be 40 c. c.

To ensure accuracy in the given results, only the purest re-agents were used and a blank estimation made with a little pure iron wire as a "fixer" for the phosphorus, gave plus the filter ash a weight of 0·0004 gram. This method is a somewhat long process, occupying nearly a day and a half, but the results obtained, even on largely-varying weights of steel, are very accurate and concordant.

The method by which phosphorus is usually estimated in the laboratories of basic steelworks, where analysis has to keep pace with the production, occupies only from one hour to two hours. It is undoubtedly a valuable process for a practical guide, but the results are not accurate; at any rate, when so low a proportion of phosphorus as 0·05 per cent. has to be dealt with. In this process the absence of silicon is pre-supposed. It is carried out thus:—

Dissolve 2 grams of steel in 20 c.c. of strong nitric acid, boil well, add a little water, boil again; next add 7 c.c. of strong hydrochloric acid, and again boil well; cool the liquid, and throw down all the iron with strong ammonia, adding 5 c.c. excess; just take up with strong nitric acid, and add 4 c.c. excess, boil well, cool slightly, and add 15 c.c. of a 10 per cent. solution of ammonic molybdate, shake the liquid thoroughly, and gently heat for a quarter of an hour, filter off the yellow precipitate, wash it with nitric acid water, dry gently, brush it off the filter, and weigh. The precipitate contains 1·63 per cent. of phosphorus. The small percentage of the element in the yellow salt makes comparatively large errors in weighing affect the result but little; but in spite of this fact, and that the salt is apt to absorb moisture in weighing, and that any silicon present in the steel, either as slag or combined, is weighed in the form of oxide as phosphorus salt,[1] the method gives low results; in fact, 25 per cent. of the total phosphorus is missed when only 0·05 per cent. is present in the steel. Thus in Casts 245 and 345 the phosphorus in each was determined five times, three times by the rapid process and twice by the slow method.

RESULTS.—CAST 245.

Slow Method.		Rapid Method.	
Per cent.		Per cent.	
(A) 0·057	Mean = 0·056 phosphorus.	(A) 0·044	Mean = 0·043 phosphorus.
(B) 0·054		(B) 0·042	
		(C) 0·044	

Rapid method 0·013 per cent. low.

CAST No. 345.

Slow Method.		Rapid Method.	
Per cent.		Per cent.	
(A) 0·049	Mean = 0·050 phosphorus.	(A) 0·037	Mean = 0·037 phosphorus.
(B) 0·051		(B) 0·036	
		(C) 0·039	

Rapid method 0·013 per cent. low.

The filtrates from both (B) results of the rapid process were carefully examined, and in each case the presence of a small amount of phosphorus was distinctly proved, the pyrophosphate obtained weighing in either result about 1 milligram.

Regarded from a practical point of view, the error is not of much moment, and when the percentage of phosphorus rises above 0·10, the relative proportion of loss is less; still, the usual tendency of results obtained by the rapid process is to indicate less phosphorus than is really present, and returns of 0·01 and 0·02 per cent. of P. should be taken *cum grano salis*. The error is, moreover, readily increased by slight violations of the conditions of analysis. On the other hand, if the temperature of precipitation is too high, approaching boiling point, molybdic acid is obtained, in addition to phospho-molybdate; this may either bring the result nearer the truth, or may make it high according to the amount of molybdic acid thrown down, which is quite uncertain; so that a method containing so many points of uncertainty, and in which so minute a weight of the element is precipitated in the presence of such comparatively enormous weight of foreign matter, is not to be recommended when scientific accuracy is desired.

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[1] As a rule, however, silicon is practically absent.

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"Experiments on the Transmission of Heat."

By GUSTAV ADOLPH HAGEMANN, of Copenhagen.

Translated by E. ROBERTSON.

THE problem of constructing steam-heating apparatus of given efficiency is one frequently met with in both the mechanical and chemical arts. The marine-engine surface condenser is one among many examples in which this problem has to be solved; but it is, perhaps, in the apparatus employed in sugar-making and refining that this question presents itself most frequently, and it was to obtain information in connection with this branch of the subject that the experiments about to be described were undertaken.

The Newtonian law, that the transmission of heat is in direct proportion to the difference in temperature, received in the year 1818 a great blow from Dulong and Petit's celebrated work, in which these scientific men, in a series of experiments embracing cooling in vacuo and open air, demonstrated that cooling took place more rapidly at higher temperature-differences than at lower. Later on Narr and others criticised Dulong and Petit's work, and found the divergence from the Newtonian law to be less than stated; and lastly, Colding in a treatise dated 1864 proved the complete correctness of Newton's law. Whatever then can be said for or against the deductions of the experimenters referred to, they all involve the same error, viz., that of deducing a law of universal application from too small a series of experiments, in which factors having an undoubted influence were omitted.

Possibly led away by the general acceptance of this law, possibly for want of a better starting-point, engineers have brought this law to bear upon the present problem, and thus it is found in text-books as the basis of tables and designs. Satisfaction, however, has seldom been obtained with apparatus constructed on these lines, and the result has been a mistrust of all data other than those obtained from pure experience.

In the class of apparatus in use in the sugar industry, there is the choice of heating surfaces in the form of "double bottoms," "coils," or "multitubular" arrangements, according to the conditions obtaining, the efficiency of each form varying materially, although all are supposed to come under the law that the transmission of heat is proportional to the temperature-difference.

Although the great divergencies in the results obtained from the comparatively few experiments, particulars of which have been made public, contain a sufficient challenge for an exact inquiry into the value of each of the different forms of heating-surface referred to, questions of economy limited the present inquiry to the multitubular form of heating surface. Besides the purely practical results obtained from marine-engine condensers, the experiments made by Messrs. Easton and Amos of London appear to be the only ones on this form of heating surface, the results of which have been published.

The experiments referred to were made with an apparatus consisting of a perpendicular iron tube, 1·5 millimetre ($\frac{1}{16}$ inch) thick and 38 millimetres (1½ inch) in diameter, which was enclosed in a larger tube 77 millimetres (3 inches) in diameter, containing water; dry steam of an ascertained temperature was led into the inner pipe, and the quantity of steam condensed, as well as the increased temperature of the water was measured. By means of this apparatus a temperature-difference of 69·5° Centigrade gave a heat-transmission of 15·3 units of heat per unit of area. Such an apparatus, however, could not be expected to give trustworthy results, more especially as the experiment was carried out under conditions so different from those obtaining in actual work.

With the kind assistance of Messrs. Burmeister and Wain, engineers, Copenhagen, an apparatus considered more likely to give correct results than that of Messrs. Easton and Amos, and more in accordance with those in actual use, was constructed, and is described below. A brass tube of 2 millimetres ($\frac{3}{32}$ inch) thickness of metal, and 49 millimetres (2 inches) outside diameter, and 0·941 millimetre (37 inches) length = a heating surface of 0·1330 square metre, or 1·431 square foot, was fitted steam-tight in the centre of a strong cast-iron pipe, about 6 inches in diameter inside, the lower end of the brass tube communicated by means of a pipe provided with two valves with a tank, in which was placed a copper steam-coil, whereby the water in the tank could be raised to any required temperature. The brass tube extended through the cover of the cast-iron outer pipe, and was connected to a pipe, which could easily be turned on the perpendicular axis, so that without loss of time the water coming from the tank could be either allowed to run away or be collected in the vessel on the scales. At two places' delicately adjusted thermometers were fixed. The iron-pipe could be filled with steam through a smaller pipe, provided with a tap and a lateral discharge, and had itself a discharge pipe provided with a tap.

Thermometers gave the temperature in the pipe, and this was controlled by an ordinary manometer.

T being the temperature of the steam; t_1 and t_2 the temperatures of the in- and outflowing water;

Then $T - \frac{t_1 + t_2}{2}$ is the temperature-difference in Centigrade-degrees.

If A is the weight of water in lbs. passing in θ minutes, then—

$\frac{A}{\theta}(t_2 - t_1)$ = units of heat transmitted per minute in pound-degrees (Centigrade).

$\frac{2A}{\theta} \cdot \frac{t_2 - t_1}{2T - t_1 - t_2}$ = units of heat transmitted per degree (Centigrade) of temperature-difference per minute.[1]

Lastly, the internal heating surface being O square feet.

$C = \frac{2A(t_2 - t_1)}{\theta O (2T - t_2 - t_1)}$ = units of heat transmitted per square foot per degree (Centigrade) of temperature-difference per minute.

With this apparatus, which it will be noticed is as similar as possible to that used in practice, the experiments were commenced, the result of which will be found in the following Table, and

No. of Experiment.	Steam.		Water		Temperature-difference.	Quantity of Water in lbs.	Time.		Heat-units transmitted per Minute.	Apparent Temperature-difference Centigrade.	Heat-units transmitted 1° Difference, 1 Minute, 1 Square Foot.	Velocity of Water in Feet per sec.	Reference No. to Table and letter to Plate.
	Pressure in lbs. per sq. in.	Temperature Centigrade.	At Inflow.	At Outflow.			′	″					
1	0	100·0	5·7	46·7	41·0	46·3	3	0	632·7	86·5	5·12	0·20	1. A.
2	9	115·1	5·3	36·7	31·4	104·3	3	0	1,089·1	96·7	5·98	0·56	
3	30	133·0	5·3	46·1	40·8	100·3	3	0	1,355·8	110·0	7·54	0·49	

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[1] The Centigrade temperatures given in the original memoir have been retained partly for convenience, partly because Centigrade temperatures are now most commonly used in scientific investigations. Consequently the unit of heat is the heat necessary to raise 1 lb. of water 1° Centigrade. It may be noted that the numbers in the columns below headed "Heat-units transmitted, 1° (Centigrade) difference, 1 minute, 1 square foot," are also identical with the heat-units transmitted in pound-degrees Fahrenheit, for 1° Fahrenheit of temperature-difference, per minute and per square foot.

graphically represented by curve A, Plate 7, Fig. 2. In this plate temperature-differences are set off as abscissas, the pound-degrees transmitted as ordinates. The steam-pressure and the corresponding temperature vary while the temperature of the inflowing water is kept constant. The resulting line is tolerably continuous, and seems at the beginning to keep near to Newton's law, but afterwards to follow Dulong and Petit's, or rather Narr's law, as the transmission rises in a somewhat greater proportion than the temperature-differences.

Soon after the commencement of the experiment, it was noticed that the thermometer did not show a constant temperature, and, even taking the average of many observations, did not give the same temperature as the water in the collecting vessel, but a much lower one. The thermometer was now moved from its central position to the periphery of the pipe, when it was found that the water was running through in layers, of which that nearest the periphery was the hottest, consequently the temperature of the water in the collecting vessel was not the average temperature of the layer that touched the inner side of the working surface, but a lower one, and $T - \frac{t_2 + t_1}{2}$ was not the difference between the two sides of the working surface, but a larger figure. It need scarcely be said that under such circumstances the value given for C in Table 1 is wrong, and the curve also.

The reason attention is drawn to this mistake is that the same mistake was made by both Dulong and Petit and Colding, and because it is one into which so many other experimenters have fallen. Dulong and Petit experimented on the cooling of a highly heated thermometer-bulb placed in a copper ball surrounded by water, but in calculating the temperature-differences, they apparently neglected the effect not only of the air currents set up inside the ball, but also the water-currents on the outside of it. Narr employed ice for cooling the copper ball in repeating this experiment for the purpose of eliminating the effect of the water-currents.

Colding, who experimented on the cooling of water flowing through pipes placed horizontally, draws attention to the fact that the temperature of the water was higher at the top than it was at the bottom of the pipe, without noting, however, that if this was true for the less mobile water, how much more must the more easily moved air surrounding the pipe have been affected.

To avoid troublesome corrections which would otherwise have been necessary to obtain true temperature-differences with the experimental apparatus already described, a tube 38·5 millimetres (1½ inch) in external diameter, and closed at both ends, was placed in the heating tube, thus reducing the space through which the water passed to an annular area 3·25 millimetres ($\frac{1}{8}$ inch) in width. Preliminary experiments with the altered apparatus showed that the thermometer gave very constant readings, and they corresponded closely with those of a thermometer placed in the collecting vessel.

A series of experiments was now undertaken to determine the influence which speed of flow had upon transmission when the temperature-differences were constant, and the steam temperature was maintained at 100°. The results are found in Table 2, and are graphically presented by the darker curve, Plate 7, Fig. 1, where the velocities of the water in feet per second are set off as abscissas, and the corresponding heat-transmission as ordinates. The points shown by small circles are determined by measurement, and the points shown by large circles are borrowed from lines found by other experiments. It will be noted that a speed of 0, instead of giving no transmission, gave a very perceptible amount, which was caused by the streams generated in the water, the same circulation which renders possible a comparatively rapid heating of water, in spite of its being a bad conductor.

At a temperature-difference of 51°, a speed less than that due to heating alone has but little influence, but with a greater speed the influence is very marked, the transmission increasing rapidly, though not quite in proportion to the speed. Of the early experimenters, only Colding attempted to determine the influence of speed on cooling, the experiments being made with the same apparatus as those already alluded to, in the open air under varying conditions of wind and weather; the disagreements in the results were supposed to be due to the effect of the wind, and from such rough approximation was deduced a law for the influence of currents.

After determining the influence of speed at a temperature-difference of 51°, another series of experiments was undertaken to find the influence of temperature-differences at constant speed. Tables 3, 4, 5, and 6, give the results of these experiments, which are graphically represented on Plate 7, Fig. 2, curves B, C, D, and E.

No. of Experiment.	Steam.		Water		Temperature-difference.	Quantity of Water in lbs.	Time.		Heat-units transmitted per Minute.	Apparent Temperature-difference Centigrade.	Heat-units transmitted 1° Difference 1 Minute 1 Square Foot.	Velocity of Water in Feet per second.	Reference No. to Table and letter to Plate.
	Pressure in lbs. per sq. in.	Temperature Centigrade.	At Inflow.	At Outflow.									
		°	°	°	°		Min.	sec.		°			
4	0	100	26·1	70·6	44·5	25·4	5	0	225·7	51·7	3·04	0·30	2. A.
5	0	100	25·8	70·7	44·9	25·4	5	0	227·5	51·8			
6	0	100	32·2	63·7	31·5	39·0	5	0	250·6	52·2	3·29	0·46	
7	0	100	32·2	63·4	31·2	38·6	5	0	240·7	52·2			
8	0	100	37·0	58·8	21·8	77·6	5	0	338·2	52·1	4·41	0·89	
9	0	100	36·8	58·7	21·9	72·3	5	0	316·6	52·1			
10	0	100	41·3	56·0	14·7	88·6	2	0	651·2	51·4	8·75	2·60	
11	0	100	41·2	56·0	14·8	86·9	2	0	643·8	51·4			
11*a*	0	100	41·3	52·9	11·6	101·4	1	30	757·9	52·9	9·99	4·06	
11*b*	0	100	42·8	51·4	8·6	95·7	1	0	826·0	52·7	10·94	5·57	
11*c*	0	100	41·2	50·5	9·3	113·5	1	10	904·7	54·2	11·65	5·67	
11*d*	0	100	50·7	58·0	7·3	99·6	1	0	729·7	45·7	11·14	5·93	
12	0	100	88·9	93·6	4·7	85·5	5	0	80·2	8·8	6·35	1·02	3. B.
13	0	100	86·4	92·0	5·6	85·5	5	0	95·7	10·8	6·17	1·02	
14	0	100	73·4	83·9	10·5	86·4	5	0	183·4	21·3	5·98	1·03	
15	0	100	56·8	72·7	15·9	86·4	5	4	275·0	35·3	5·43	1·03	
16	0	100½	47·8	66·9	19·1	87·5	5	0	334·0	42·7	5·45	1·04	
17	0	100½	40·0	61·4	21·4	69·9	4	0	368·1	49·3	5·19	1·02	
18	0	100½	35·5	57·3	21·8	86·4	5	0	377·1	53·6	4·90	1·03	
19	0	100½	28·9	51·7	22·8	85·3	5	0	389·5	59·7	4·55	1·02	
20	0	100½	15·9	41·7	25·8	87·5	5	0	452·1	72·2	4·36	1·01	
21	0	100	88·5	92·1	3·6	75·0	2	0	137·9	9·7	9·66	2·23	4. C.
22	0	100	88·0	91·7	3·7	73·6	2	0	136·0	10·2	9·29	2·19	
23	0	100	86·0	90·4	4·4	70·3	2	0	154·5	11·8	9·14	2·09	
24	0	100	70·5	79·8	9·3	70·3	2	0	326·6	24·9	9·14	2·07	
25	0	100	70·3	79·3	9·0	69·9	2	0	314·3	25·2	8·69	2·08	
26	0	100	55·1	67·8	12·7	69·9	2	0	443·5	38·6	8·17	2·08	
27	0	100	40·5	56·0	15·5	70·3	2	0	544·9	51·8	7·29	2·09	
28	0	100	30·7	46·2	15·7	70·5	2	0	553·8	61·7	6·22	2·10	
29	0	100	9·8	28·6	18·8	69·9	2	0	656·7	80·8	5·65	2·08	
30	0	100	90·8	93·0	2·2	56·6	1	0	124·6	8·1	10·14	3·35	
31	0	100	90·8	92·8	2·0	56·2	1	0	112·4	8·2			5. D.
32	0	100	85·4	89·2	3·8	65·0	1	10	211·8	12·7	11·59	3·31	
33	0	100	60·5	70·0	9·5	57·5	1	0	546·0	34·8	10·90	3·41	
34	0	100	43·5	56·5	13·0	57·1	1	0	741·2	50·0	10·34	3·40	
35	0	100	9·5	25·3	15·8	56·6	1	0	895·1	82·6	7·53	3·38	
36	0	100	89·7	91·4	1·7	90·4	1	0	159·2	9·5	11·68	5·38	6. E.
37	0	100	72·0	78·2	6·2	91·4	1	0	567·2	24·9	15·90	5·44	
38	0	100	52·8	61·6	8·8	107·4	1	10	809·0	37·8	14·91	5·46	
39	0	100	9·6	22·2	12·6	90·4	1	0	1,138·9	84·1	9·53	5·38	
40	0	100	9·6	21·5	11·9	77·2	0	50	1,102·3	78·9	9·66	5·50	

It will at once be seen that on the first portions of the curves the necessary data for determining them are wanting; that 0° temperature-difference must give 0 pound-degree transmission is sufficiently clear. From 0 to 8·8° the determinations are wanting, because the steam coil was not able to keep a higher temperature in the tank than 88·9°. The first part of the curves, however, may reasonably be taken as correct, at least as far as concerns the general direction, the possibility of this being otherwise being much reduced by an examination of the results of numerous experiments, with various water-speeds, and by comparison with the boiling or evaporation curve F. From the foregoing it appears that heat-transmission per degree is greatest at low temperature-differences, and it is unfortunate that circumstances prevented the determination of the point at which maximum transmission was reached. It is clear, however, that the maximum point is reached when the heat, carried away by the water, reaches the amount which the thermal conductivity of the heating surface can yield under the given conditions. In such a case the heating surface must be understood to be the metal tube which, on the steam side, has a constant film of water, which continually runs down it and is constantly being re-formed.

The curve of transmission falls gradually back from the maximum point proportionally with the temperature-difference, and although many of the determinations, especially those of curve B, do not follow the curve as drawn, still the determinations undoubtedly give the direction of the curve, and the discrepancies are not to be wondered at, when the great difficulties these determinations present are taken into account.

Here it should be remembered that so far the presupposition that $\frac{t_1 + t_2}{2}$ is the true mean temperature of the water during its course through the heating tube has not been confirmed, it does not hold good near and immediately at the maximum point. As the maximum point is approached, $\frac{t_1 + t_2}{2}$ is too low, giving too high a temperature-difference, with a corresponding effect on the result, and this is equally true on both sides of the maximum point.

Indirect evidence can be brought to show the form of curve that can with most safety be taken as correct, and with this end in view a series of experiments was undertaken on the evaporative power of the tube, in the following manner. The upper end of the tube was provided with a pipe communicating with a vessel, partially closed by two deflectors, intended to prevent priming. From this a rubber pipe led to a smaller glass vessel, where the water-level could be measured by a fixed glass peg. As soon as boiling began the water rose in the small vessel to make room for the steam in the pipe, and when the evaporation had caused the water to reach the glass peg, time was noted, one or two litres of water were successively added, and the time when the water-level reached the glass peg was again noted, by which means the heat-units and time occupied were determined.

As water boils at a constant temperature of 100°, there was no question as to mean temperature, and the temperature-difference is got by substracting 100 from the temperature of the steam. Table 7 and curve F show the results obtained.

No. of Experiment.	Steam.		Water Evaporated.		Time.	Temperature-Difference.	Heat-units transmitted 1° Difference, 1 Minute, 1 Sq. Foot.	Reference No. to Table, and letter to Plate.
	Pressure in lbs. per sq. in.	Temperature.	lbs.	Temperature.				
		°		°	Min. Sec.	°		
41	..	106	1·102	33	7 30	6·0	10·36	7. F.
42	..	106	1·102	30	7 23			
43	1	108	1·102	26	4 12	8·0	14·28	
44	1	107¾	1·102	24	4 10			
45	1	108	1·102	25	4 0			
46	7	111	1·102	25	2 35	11·0	17·20	
47	7	110¼	2·204	25	5 0			
48	8	112	2·204	25	4 37			
49	9	115	2·204	24	3 23	15·0	18·02	
50	20	126	2·204	24	2 43	26·0	13·44	
51	20	126	2·204	22	2 43			
52	30	133½	2·204	19	2 50	34·2	9·98	
53	33	135	2·204	19	2 51			
54	55	150	2·204	19	3 17	50·2	5·90	
55	55	151	2·204	19	3 3			

With low temperature-differences the boiling is apt to be jerky and explosive in character, and mistakes are easily made; also, the long time the experiment takes reduces the transmission. Curves for boiling show the maximum point formed very much as curves B and C. After the maximum point is reached the line falls rapidly. It must here be noted that high temperature-differences caused the boiling to assume the same character as that at low temperature-differences. This is most probably caused by the steam forming more rapidly in the tube than it can escape from it, a circumstance which must have had an influence on the direction of the curve.

It may be assumed, therefore, that the curves D and E should have a form that more resembled B and C, which, together, will give a law for the position of the maximum point, which might be anticipated as existing.

Meanwhile, the Author does not consider as sufficient the evidence advanced for this assumption, since at any rate one observation is decidedly against it.

So far the temperature-differences which occur between steam at 100° and water of higher or lower temperature have only been considered, and it was only in the boiling experiments that steam of higher pressure and temperature was used. Differences can also occur between steam of varying pressure and temperature and water of constant temperature, and also the constant water temperature may be higher or lower, which further investigations afford a rich field for experiments, the results of which it would be difficult to forecast with any probability from the previously described experiments.

The influence of water-speed on heat transmission is also an important factor to be considered, and the results of experiments made with that end in view are given in Tables 8, 9, 10, and 11, and curves G, H, I, and J.

No. of Experiment.	Steam.		Water		Temperature-difference.	Quantity of Water in lbs.	Time.	Heat-units transmitted per Minute.	Apparent Temperature-difference.	Heat-units transmitted 1° Difference, 1 Minute, 1 Square Foot.	Velocity of Water in Feet per Second.	Reference No. to Table and letter to Plate.
	Pressure in lbs. per Sq. In.	Temperature.	At Inflow.	At Outflow.								
		°	°	°	°		Min. sec.		°			
56	0	104·4	5·5	30·8	25·3	99·2	3 2	826	86·2	6·67	1·94	8. G.
57	10	115·0	5·7	36·5	30·8	99·2	3 9	970	93·9	7·19	1·87	
58	20	124·0	5·6	40·2	34·6	99·2	3 11	1,071	101·2	7·37	1·83	
59	41	138·2	5·6	47·2	41·6	99·2	3 9	1,309	111·3	8·19	1·87	
60	0	100·4	5·8	29·2	23·4	99·2	2 46	844	82·9	7·07	2·14	9. H.
61	0	100·5	5·8	29·8	22·0	99·2	2 46	864	82·7	7·21	2·14	
62	8	112·8	6·0	35·4	29 4	99·2	2 47	1,060	92·1	7 98	2·14	
63	14	119·8	6·0	38·0	32·0	99·2	2 45	1,153	97·8	8·20	2·14	
64	16	122·5	6·0	39·0	33·0	99·2	2 45	1,190	100·0	8·30	2·14	
65	32	133·2	6·0	44·2	38·2	99·2	2 45	1,378	108·1	8·91	2·14	
66	0	100·5	6·0	26·2	20·2	99·2	2 0	1,003	84·5	8·30	2·95	10. I.
67	0	107·0	6·0	28·6	22·6	99·2	2 0	1,120	89·7	8·79	2·95	
68	11	116·0	6·0	31·0	25·0	99·2	2 0	1,239	97·5	8·83	2·95	
69	14	119·4	6·0	32·4	26·4	99·2	2 0	1,309	100·2	9·05	2·95	
70	29	131·5	6·0	35·8	29·8	99·2	2 0	1,477	110·6	9·27	2·95	
71	29	131·0	6·0	35·6	29·8	99·2	2 0	1,477	110·3	9·27	2·95	
72	0	100·0	6·4	20·8	14·4	77·1	0 59	1,111	86·4	8·91	4·59	11. J.
73	0	100·0	6·5	20·8								
74	0	105·0	6·8	22·2	15·4	77·1	0 59	1,188	90·5	9·14	4·59	
75	9	113·2	6·8	23·6	15·8	77·1	0 59	1,296	98·0	9·21	4·59	
76	22	126·0	6·5	26·6	20·0	77·1	0 59	1,543	109·1	9·89	4·59	
77	22	125·0	6·4	26·2								
78	28	130·5	6·3	26·6	20·3	77·1	1 0	1,565	113·6	9·48	4·59	
79	28	129·4	6·3	26·6								

It is a natural result of the method of experiment adopted that the parts of the curves of transmission actually determined are not greater, and begin rather high up, but the principal result obtained previously, namely, that the transmission of heat increases with increasing speed is further confirmed. In order to make some determinations with lower temperature-differences, an air-pump was connected to the steam chamber of the apparatus to produce a partial vacuum, so as to obtain measurements with lower steam temperature. The experiment was, however, only partially successful, because the pump and condenser were too small, but the results given in Table 12, curve K, confirm the correctness of the previous experiments.

No. of Experiment.	Steam.		Water		Temperature-difference.	Quantity of Water in lbs.	Time.	Heat-units transmitted per Minute.	Apparent Temperature-difference.	Heat-units transmitted 1° Difference 1 Minute 1 Square Foot.	Velocity of Water in Feet per Second.	Reference No. to Table and letter to Plate.
	Pressure in lbs. per sq. in.	Temperature.	At Inflow.	At Outflow.								
		°	°	°	°		Min. sec.		°			
80	22	127·0	6·4	40·6	34·2	77	2 15	1,173	103·5	7·91	2·04	12 K.
81	19	123·6	6·5	39·4	32·9	77	2 15	1,129	100·7	7·74	2·04	
82	15	120·0	6·5	37·5	31·0	77	2 15	1,063	98·0	7·52	2·04	
83	6	113·2	6·5	34·8	28·3	77	2 15	970	92·6	7·29	2·04	
84	1	105·0	6·3	31·0	24·7	77	2 15	847	86·4	6·61	2·04	
85	0	100·0	6·5	28·2	21·7	77	2 12	761	82·7	6·37	2·08	
86	0	97·0	6·5	26·8	20·3	77	2 14	692	80·3	6·00	2·04	
87	0	95·5	6·5	26·6	20·1	88	2 32	699	79·0	6·14	2·04	
88	0	88·0	6·5	23·8	17·3	77	2 15	593	72·9	5·63	2·04	

It will at once be noted that in this series of experiments the amount of heat transmitted does not rise as rapidly with the speeds as in the first series, which may be accounted for by a slight incrustation detected on the inner side of the heating tube resulting from previous experiments, together with, perhaps, errors of observation. The effect of the incrustation was determined, and another series of experiments undertaken (see Table 16 and curve O), but while noting the error no reliable expression can be given for its quantitative influence on the results.

With the same speed, a changing temperature-difference can occur when the temperature of both the inflowing water and the steam is increased (see Table 13 and curve L).

These results, compared with those given in Table 8, curve G, show how a higher temperature of the inflowing water, with the same steam temperature, increases the transmission.

No. of Experiment.	Steam.		Water		Temperature-difference.	Quantity of Water in lbs.	Time.	Heat-units transmitted per Minute.	Apparent Temperature-difference.	Heat-units transmitted 1° Difference 1 Minute 1 Square Foot.	Velocity of Water in Feet per Second.	Reference No. to Table and letter to Plate.
	Pressure in lbs. per sq. in.	Temperature.	At Inflow.	At Outflow.			Min. sec.					
89	0	106·1	14·6	39·6	25·0	99·2	3 7	794	79·0	7·00	1·93	13. L.
90	11	115·5	14·8	44·8	30·0	99·2	3 15	915	85·7	7·45	1·82	
91	20	124·6	13·6	47·4	33·8	99·2	3 15	1,032	94·1	7·64	1·82	
92	41	138·4	12·6	54·4	41·8	99·2	3 14	1,274	104·9	8·47	1·82	
93	6	109·6	60·0	70·2	10·2	99·2	1 52	551	44·5	8·60	3·19	14.M.
94	6	109·8	59·4	69·8	10·4	99·2	1 52	551	44·5	8·60	3·19	
95	17	122·5	59·5	74·0	14·5	99·2	1 53	756	53·7	9·44	3·19	
96	17	122·3	59·4	74·2	14·8	99·2	1 53	772	55·8	9·50	3·19	
97	31	133·2	59·0	77·0	18·0	99·2	1 51	966	65·2	10·28	3·19	
98	36	135·0	58·8	77 6	18·8	99·2	1 51	1,007	66·8	10·34	3·19	
99	36	136·0	58·8	78·0	19·2	99·2	1 51	1,030	67·6	10·53	3·19	
100	36	136·2	80·2	98·2	18·0	99·2	2 13	811	47·0	11·51	2·67	15*a*. N. N. N.
101	0	136·6	82·0	100·0	18·0	99·2	2 16	787	45·6	11·98	2·62	
102	0	136·6	75·0	94·0	19·0	99·2	2 13	855	52·1	11·45	2·67	
103	0	135·8	60·0	80·2	20·2	99·2	2 8	941	65·7	9·83	2·75	
104	0	136·4	58·5	79·6	21·1	99·2	2 7	996	67·4	10·28	2·77	
105	0	136·3	52·0	74·0	22·0	99·2	2 7	1,038	73·2	9·91	2·77	
106	0	135·5	48·6	71·0	22·4	99·2	2 6	1,058	75·7	9·75	2·78	
107	0	136·2	45·2	68·4	23·2	99·2	2 7	1,096	79 4	9·60	2·77	
108	0	136·0	39·4	64·0	24·6	99·2	2 7	1,162	84·3	9·60	2·77	
109	0	136·6	36·5	62·0	25·5	99·2	2 7	1,252	84·4	9·97	2·77	
110	0	136·8	31·6	58·0	26·4	99·2	2 7	1,246	92·0	9·44	2·77	
111	0	136·8	15·3	43·6	28·3	99·2	2 8	1,318	106·4	8·60	2·75	
112	0	136·4	6·0	36·8	30·8	99·2	2 8	1,433	115·0	8 68	2·75	
..	4	109·4	7·6	28·8	21·2	99·2	2 1	1,041	91·2	7·90	2·92	15*b*. *n*. *n*.
..	..	110·0	7·0	28·0	21·0	99·2	2 2	1,025	92·5	7·74	2·89	
..	..	110·0	34·4	50·8	16·4	99·2	..	800	67·4	8·20	2·89	
..	..	110·0	31·8	49·2	17·4	99·2	2 4	833	69·5	8·30	2·89	
..	..	110·4	38·5	54·8	16 3	99·2	..	780	63·8	8·52	2·89	
..	..	110·4	43·0	58·2	15·2	99·2	2 3	734	59·8	8·60	2·89	
..	..	110·0	45·2	60·0	14·8	99·2	2 3	716	57·4	8·68	2·89	
..	..	100·0	59·3	70·0	10·7	99·2	1 54	567	45·4	8·68	3·18	
113	0	100·0	6·6	24·0	17·4	77·2	1 30	899	84·7	7·39	3·05	16. O.
114	..	100·0	6·6	24·2	17·6	77·2	1 30					
115	11	116·0	6·7	28·8	22·3	77·2	1 32	1,124	98·5	7·94	3·00	
116	..	116·5	6·6	29·0								
117	16	121·8	6·5	30·4	24·1	77·2	1 31	1,224	103·5	8·22	3·02	
118	..	122·2	6·5	30·8								
119	22	127·0	6·5	32·2	25·7	77·2	1 31	1,312	107·7	8·47	3·02	
120	..	127·0	6·5	32·2								
121	33	134·0	6·5	34·0	27·5	77·2	1 31	1,391	114·0	8·49	3·02	
122	..	134·5	6·5	34·0								

Table 14, curve M, gives a part of a curve that corresponds to a temperature of about 59° for the inflowing water, and varying steam pressure. Tables 15*a* and 15*b*, curves N *n*, give with curves N_1 and n_1 a law for this increase of transmission with water temperatures, similar to those in curves B, C, D, E.

Curves N_1 n_1 are higher than those last-mentioned because they are produced with a steam temperature of 110° and 136°, as against a temperature of 100°. Curve N_1 should cut the boiling-curve F at 36° temperature-difference; and as it does not do so exactly, it is probable that curve F ought to follow the direction of the dotted line as previously suggested.

The foregoing are the results of a series of careful experiments on some of the conditions affecting the heat-transmitting power of one form of heating surface, from which the experimenter hopes practical men may be able to obtain at any rate some little information.

No attempt is made to deduce a general law from these experiments, owing to their comparatively limited range, and to the neglect of several important factors, such as the form and position of the heating surface, the thermal conductivity of the metal forming it, and the specific heat of the liquid receiving the heat.

The communication is illustrated by two diagrams, reproduced in Plate 7.

(*Paper No. 1985.*)

"On Galvanic Action between Wrought-Iron, Cast Metals, and various Steels during long Exposure in Sea-Water."

By THOMAS ANDREWS, F.R.S.E., ASSOC. M. INST. C.E.

IN this Paper the Author has endeavoured to investigate some of the conditions attending galvanic action between wrought-iron, cast metals, and various steels in sea-water, inasmuch as the accurate measurement of the electro-chemical positions of these metals when in combination should afford an indication of their relative corrosibility under such circumstances in sea-water. The Author has therefore considered it essential to make observations on a large scale, to obtain information as to the ultimate permanent electro-chemical positions assumed by these metals after long periods of exposure; also to ascertain whether the interchange of the electro-chemical positions noticed in course of the experiments was a recurring condition attending galvanic action between these metals in sea-water.

In the following experiments the samples of wrought-iron, steels, cast metals, &c., employed were in round bars, specially manufactured with reference to the percentages of combined carbon. Specimens of the highest and lowest percentages have been chosen, as calculated to give the alternate extreme results in each case.

Round bars were selected in preference to plates to ensure a more uniform molecular structure, pressure being in their manipulation thus exerted towards the centre.

The terms "soft steel" and "hard steel" in this Paper are used simply as distinctive terms referring to differences arising from the higher or lower percentages of combined carbon in the metals under observation.

The chemical composition and specific gravity of the metals used are given in Table I.

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[1] Combined carbon in these samples was determined by combustion, and in the other samples by the colour test.

TABLE I.—ANALYSES of the WROUGHT IRONS, VARIOUS STEELS, and CAST METALS EMPLOYED.

Description.	Graphitic Carbon.	Combined Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Tungsten.	Iron (by difference).	Total.	Specific Gravity.
Wrought iron (Wortley Best Scrap) . . .	..	None	0·392	·034	·270	·194	..	99·110	100	7·590
Wrought iron (Lowmoor).	..	0·114	0·146	·013	·107	·159	..	99·461	100	7·720
"Soft" Bessemer steel	..	0·150	0·009	·112	·088	·468	..	99·173	100	7·853
"Soft" Siemens-Martin steel	..	0·230	0·014	·100	·075	·698	..	98·883	100	7·856
"Soft" cast steel . .	..	0·450	0·016	·027	·048	·086	..	99·373	100	7·863
"Hard" Siemens-Martin steel	..	0·460	0·107	·023	·075	·972	..	98·363	100	7·845
"Hard" Bessemer steel	..	0·480	0·121	·096	·089	·684	..	98·530	100	7·838
"Hard" cast steel .	0·259	1·190[1]	0·175	·063	·019	·396	..	97·898	100	7·805
Cast metal No. 1 . .	2·780	0·390[1]	2·340	·090	·580	·450	..	93·370	100	7·206
Cast metal No. 2 . .	2·620	0·670[1]	1·940	·090	·950	·520	..	93·210	100	7·134
Tungsten steel . .	..	1·750[1]	0·135	·069	·139	·720	9·270	87·917	100	..

The wrought-irons were made at the Author's works (the Wortley Iron Works) from selected clean scrap-iron, with exception of the Lowmoor.

Cast metal No. 1 was a mixture of one-half of Lilleshall Cold Blast No. 3, and one-half of Hematite No. 3.

Cast metal No. 2 was a mixture of one-half Scrap metal, and one half Sheepbridge No. 3.

Every bar was 13 inches long, accurately turned to exactly 3 inches diameter, and polished, so that each had a surface area of 84·823 square inches exposed to the action of the sea-water. The Author purposely had the bars turned perfectly clean in the lathe and polished in preference to cleaning them by immersion in acid, as he is of opinion that the action of the acid would in a galvanic research of this kind materially alter and destroy the peculiar and somewhat complex molecular structure of the steels and other metals under observation. The Author inclines to the opinion that the molecular structure of steels is of a more compound character than is generally supposed, a view which microscopical investigation tends to confirm.[1]

A bar of each of the metals was immersed in an equal quantity of sea-water in one of the cells (Fig. 1), at distances of ½ inch apart, and connected with a bar of bright polished wrought-iron, the circuit being completed by attachment to a sensitive galvanometer.

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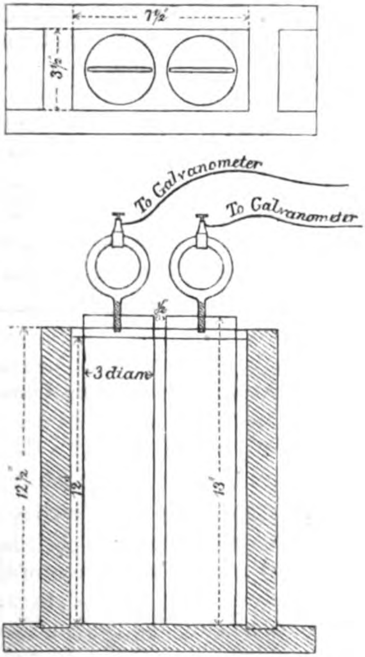
[1] Researches of Mr. H. Clifton Sorby, LL.D., F.R.S., Sheffield; also those of Mr. A. Martens, Berlin, and that of Mr. J. C. Bayles, New York, and others.

Each of these wrought-iron bars used as a standard in connection with each steel or other bar, was, as far as practicable, of the same nature and chemical composition, being a portion cut from one large bar of uniform structure. Other portions of this large standard wrought-iron bar were arranged in couples and immersed in sea-water for long periods, to ascertain whether any galvanic action took place between them likely to interfere with results, and on the completion of the research, bars of the wrought-iron which had been used in the experiments were re-turned and polished, and immersed in circuit for long periods in sea-water, when a very feeble action only was observed, not sufficient to appreciably affect any of the results recorded in this Paper.

FIG. 1.



CELLS EMPLOYED.
Scale, 1½ inch = 1 foot.

The observations during the earlier part of the time were taken with a single-needle galvanometer, with jewelled centre, the resistance of this instrument being 220 ohms. During the latter half they were taken with a very sensitive astatic galvanometer by Elliott Bros., having suspended needles, and provided with a long delicate pointer, enlarged mirror dial, &c.; the resistance of this galvanometer was, at 20° Centigrade, 521 ohms. Both galvanometers were twice carefully calibrated with two separate standard Daniell's elements in circuit, their constancy was frequently verified, and all the readings throughout this research were taken with one of Elliott's reading telescopes. Through the kindness of Captain W. Huntley, a supply of pure sea-water was obtained as required from Filey Bay, from the clear deep sea.

During the first six hours of each experiment each pair of bars remained constantly attached to the galvanometer. After the first day observations were taken in the following manner, at the same time daily, of the whole series of cells conveniently arranged, containing the various metals which remained throughout undisturbed in sea-water. Each pair of iron and steels was consecutively attached for five minutes daily to the galvanometer, and the deflections then observed were recorded. The total resistance, &c., in circuit having been frequently measured, the value of the deflections of the galvanometers in volts was thus known from the accurately detailed calibration of the instruments twice made at the commencement of the research. The metals when not in circuit through the galvanometer were maintained constantly in connection by strong screw-clips and thick copper wires. The arrangement of the cells is shown in Fig. 1.

The average electrical resistance of these cells, &c., from many observations, was found to vary from 3 to 12 ohms (probably owing to the accumulation of oxides, &c.). The sea-water in the cells was carefully siphoned off and changed in course of the experiments. The sea-water in the cells, between the time of replenishing, was always maintained at the standard level by the frequent addition of distilled water. The stout connecting wire of copper was of the same diameter (0·105 inch) and of the same length, so that the resistance was equal in each experiment. Great care was exercised throughout the research, and all the conditions of whatever kind were similar.

GENERAL RESULTS.

The general results are recorded in the Appendix.

The results in Table VII., give a comparison of the relative periods of time that the metals remained in the positive or negative position, and the comparative average extent of the electromotive force, during such periods, compiled from the previous Tables and diagrams. The metals in this Table are placed in comparative order. They are arranged under the galvanic conditions recorded, taking as a guide the average electromotive force from the steels whilst in the electro-positive position.

TABLE VII.—SUMMARY of RESULTS on TABLES II., III., IV., V. and VI.

Total duration of experiments, three hundred days.

Description of Steels, &c.	Electromotive Force in Volts of the Steels, &c., whilst in the Electro-positive position.		Total Number of days the Steels, &c., remained in the Electro-positive position, connected with Wrought-Iron in Sea-water.	Electromotive Force in Volts, of the Steels, &c., whilst in the Electro-negative position.		Total Number of days the Steels, &c., remained in the Electro-negative position, connected with Wrought Iron in Sea-water.	Total Number of interchanges of Electro-chemical position.
	Highest	Average.		Highest	Average.		
Cast metal No. 2.	0·116	0·030	233	0·011	0·006	66	1
"Hard" Bessemer steel	0·063	0·022	104	0·052	0·003	101	16
Cast metal No. 1.	0·092	0·021	246	0·005	0·003	53	1
"Soft" cast steel	0·089	0·016	293	0·027	0·019	6	2
"Soft" Siemens-Martin steel	0·116	0·015	208	0·027	0·006	66	4
"Hard" cast steel	0·045	0·011	276	0·033	0·016	16	2
"Soft" Bessemer steel	0·052	0·010	216	0·015	0·007	62	4
"Hard" Siemens-Martin steel	0·084	0·007	258	0·023	0·009	28	2

Attention should be drawn to the complete interchange of electro-chemical position occurring in the case of every metal at various times during the observations.

The above results summarise the relative extent of the action of sea-water on each metal under the conditions of the experiments. A reference to Table VII. also shows that the extent of the action in many instances was very considerable, in several cases the highest varying from about the $\frac{1}{9}$ to $\frac{1}{11}$ volt, and in some others a general average of about the $\frac{1}{40}$ volt.

So far as the average electromotive force during such times as the steels and other metals were in the electro-positive position may be taken as an indication, it will be seen in what order the metals stand, as regards liability to corrosion under similar conditions to the experiments; the influences of local action must not, however, be disregarded.

By recording, however, either the deflections of the galvanometer first observed, or only the average electromotive force observed for a certain period of time, many interesting changes would be omitted which might lead to erroneous conclusions as to the relative electro-chemical position of the metals.

The Author, therefore, deemed it advisable to give the full records of the time-changes observed during extended periods; and he presents in this Paper the curves of the electromotive force observed, in the rough, without any attempt at rounding off.

On the conclusion of the experiments the bars were removed from the sea-water cells, and the loss in diameter was ascertained by a Troughton and Simm's compound fine adjustment slide and screw-gauge, measuring absolutely to $\frac{1}{1000}$ inch. The comparative diminution in size from the galvanic corrosion is given in Table VIII., these results having been obtained from the averages of a considerable number of measurements taken in each case just below the water-line, where the corrosion appeared greatest.

On comparing Table VIII. with the general summary of electric results in Table VII., it will be seen that general confirmatory results were obtained in almost every instance in favour of the wrought iron employed under the conditions recorded.

TABLE VIII.—LOSS IN DIAMETER of the BARS IMMERSED in SEA-WATER for THREE HUNDRED DAYS in GALVANIC CONNECTION.

Results in parts of 1 inch.

Description of Steels, &c.	Percentage of combined Carbon.	Specific Gravity.	Reduction in Diameter of the various Steel and Cast-metal Bars, &c., in Galvanic connection with a Wrought-iron Bar.	Reduction in Diameter of the Wrought-Iron Bars in Galvanic connection with each Steel, &c., in column No. 1.
Wrought iron	None	7·590	..	..
"Soft" Bessemer steel. . .	0·150	7·853	0·0030	Nil
"Soft" Siemens-Martin steel.	0·230	7·856	0·0020	0·0018
"Soft" cast steel	0·450	7·863	0·0004	Nil
"Hard" Siemens-Martin steel	0·460	7·845	0·0045	Nil
"Hard" Bessemer steel . .	0·480	7·838	Nil	Nil
"Hard" cast steel	1·190	7·805	0·0030	Nil
Cast metal No. 1	0·390	7·206	0·0065	Nil
Cast metal No. 2	0·670	7·134	0·0090	0·0010

These measurements do not indicate the full loss by corrosion as the indentations caused by local action cannot be fully gauged by this method.

On the conclusion of three hundred days, as an experiment, the current from a Daniell's element was passed for ten minutes through the hard cast steel cell in the same direction as the current from that cell. On disconnecting the Daniell's, and measuring only the current from the hard cast steel cell, the relative electro-chemical positions of the metals (hard cast steel connected with wrought iron) were found to be completely reversed by polarization, the wrought-iron being brought into the positive position with an electromotive force of 0·011 volt. The galvanometer needle, however, immediately commenced slowly to recede, crossed zero in a few minutes, and the metals assumed their original positions within ten minutes from detaching the Daniell's element. The latter was then re-attached, reversing the direction in which its current was sent, now opposing that from the wrought-iron and hard cast-steel cell, and the current was allowed to pass thus for ten minutes. On disconnecting the Daniell's element, and attaching the hard cast-steel cell alone to the galvanometer, the needle was deflected to an extent equal to an electromotive force of 0·018 volt, the hard cast steel being again metal positive, or 0·011 volt in excess of the original current of this cell. The deflection, however, slowly decreased until in ten minutes both metals resumed their original positions and electromotive force of 0·007 volt. This experiment shows that no permanent alteration of the relative electro-chemical position of the metals was thereby produced, beyond the temporary reversal due to polarization.

At the end of the three-hundred days the bars were taken out of the sea-water, and all the wrought-iron, steel and metal-bars were found coated with a reddish-yellow peroxide of iron, from absorption of oxygen, for a distance of about 3 inches below the water-line, the remaining portion being covered with a fine black deposit occasionally arranged in streaks, running the length of the bars.[1] The black carbonaceous deposit would have a tendency to develop an amount of local galvanic action on the surface of each bar.

On withdrawing the bars from the sea-water, a small piece of red litmus paper was placed on the deposit surrounding them, when a faint alkaline reaction was noticed. The manner in which the faint blue patches of varying intensities of colour were dispersed over the red litmus paper laid on the bars gave an indication of the nature of the distribution of the local action. The fine black deposit was rapidly removed by a small sponge saturated with absolute alcohol; this was squeezed into a bottle, and the deposit was thus preserved for examination.

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[1] A reference to a black deposit apparently of a similar kind, found on iron long submerged in sea-water, will be found in Dr. Percy's Metallurgy: Iron and Steel, p. 146.

The faint alkaline reaction noticed on the surface of the bars, is an indication that a slight decomposition of the chloride of sodium and other salts in the sea-water in contact with the metals had taken place by electrolysis; a minute portion of soda, &c., having been apparently set free, and the liberated chlorine had taken up its equivalent of iron.

A reaction of this nature might be partially influential, amongst other causes, in producing the interchange of electro-chemical position; but not wholly so, inasmuch as when the cells were refilled with fresh sea-water no alteration of the respective electro-chemical positions of the metals occurred, though in several cases an increase, and in others a decrease, of electromotive force took place for a time, owing to some of the adhering oxides having been probably washed off. The interchange of electro-chemical position was apparenty a reaction inherent to these metals when in contact in sea-water. Moreover the chemical reaction during such electrolysis would probably tend to retain the sea-water of the same composition, whilst at the same time oxidizing and destroying the metals; thus the chlorine liberated from the chloride of sodium on the surface of the metals by electrolysis would combine with its equivalent of iron; but the ferrous or ferric salts so formed would doubtless be again decomposed by the liberated soda precipitating the iron as an oxide, and afterwards resuming its combination with the chlorine.

In some instances, where the greatest galvanic action had taken place, the reaction of the sea-water in the cells was very faintly alkaline, this litmus reaction being, however, barely traceable in any case, as no material change appeared to have taken place in the sea-water in the cells.

Samples of the deposit found on some of the bars were submitted for examination to Mr. A. H. Allen, F.I.C., F.C.S., Borough Analyst, Sheffield, and the results are subjoined:—

"The residues were filtered off, and washed thoroughly till free from chlorides. They were then dried at a very gentle heat, weighed and ignited in a combustion tube in oxygen.

"The gaseous products of combustion were passed into barytas water, and the resultant precipitant of carbonate of barium filtered off. Both the precipitate and the filtrate therefrom were tested for acids of sulphur with wholly negative results. The carbonate of barium was converted into sulphate, and from the weight the quantity of carbon in the residue was calculated.

"The residue left after ignition in oxygen was weighed and then dissolved in acid. The solution was evaporated to separate silica, and then the ferric oxide and phosphoric acid were thrown down together by means of ammonia, the precipitate being weighed. After weighing it was tested for phosphoric acid, a trace of which was present in each case. Manganese was present in notable amount in the hard Bessemer steel deposit, but not in the other residues.

ANALYSIS of DEPOSIT on BARS.

Some Metals on which Deposit was found.	Percentage of final Residue left after Combustion.	Percentage of Oxide of Iron, &c. = Metallic Iron.	Carbon.	Silica in Residue.
Hard Bessemer steel	93·9	74·6 = 52·2	Lost	Trace
Wrought-iron connected with do.	83·9	54·5 = 38·1	9·1	Trace
Hard cast steel	80·0	55·1 = 38·6	14·9	Trace
Wrought-iron connected with do.	83·7	57·4 = 40·1	4·8	Trace

"The quantity of deposit on the bars was too small to allow of a more complete examination; neither, from the same reason, was it possible to ascertain the form of combination in which the iron existed in the original substance."

The galvanometer experiments, reaching over long periods, show that interchanges of electro-chemical position sometimes took place even after very considerable intervals, and that there appears a tendency to reflex action under the conditions of these observations; it is therefore doubtful whether a permanent position of rest finally ensues between the metals. The probabilities are against it, though eventually the galvanic action becomes very small from the accumulation and possible influences of these oxides, &c.

It will also be noticed that many steels are primarily in the positive position, and that they remain thus for various lengths of time, from causes probably connected with their composition, &c. They, however, assume for a time the negative position, the wrought-iron in circuit with them thus becoming metal positive. The fact of local galvanism, owing to the more complex composition of the steels, &c., ought at the same time to be taken into account.

Cast metals seem to retain the positive position for a long time when in circuit with wrought-iron, though after very prolonged exposure they are capable of assuming the negative position, as a reference to Table V. will show. This fact of the interchange is not only interesting in itself, but it tends to throw light on the internal molecular arrangement of these metals, and may help to afford information as to the conditions of structural formation in steels. It has a reference to the question of the relative corrosion of the metals in sea-water, &c., and may also tend to illustrate some of the causes of local corrosion or pitting in wrought iron steel, and cast metal. The tendency to interchanging electro-chemical position between iron and steels is an intricate question and presents many phases of interest.

Since some of the steels are in the positive position at the commencement of the experiment, and afterwards pass into the negative position, it may be inferred that the crystalline formations of their structure nearer the surface are at first more readily acted upon by the sea-water, until inner and seemingly less vulnerable portions have been reached.

The interchange of electro-chemical position cannot be wholly attributed to alteration in the composition of sea-water by the electric action set up, because when the sea-water in the cells was changed the electro-chemical positions, &c., of the metals were not affected, and also in experiments recorded in Tables II. and VI. and in other experiments by the Author, some of the interchanges occurred in course of the first few minutes; whereas in numerous instances mentioned in this Paper, it happened after the lapse of many days and even many months, the interim positions also remaining fairly constant for long periods.

It is improbable that the composition of the sea-water would be so changed by the electric action, in such a short time, as to be the sole cause in producing these alternations of the electro-chemical positions of the metals, because reflex actions sometimes took place whilst they were in the same sea-water.

It cannot be attributed to portions of magnetic oxide in or about the bars, because every possible care was exercised in their preparation to ensure freedom from impurities of this kind. Even if such a contingency were allowable, the electro-chemical position at first assumed by either metal should have remained permanent;

as, from other experiments by the Author extending over twelve months, any bar or plate covered with magnetic oxide, constantly maintains the negative position, compared with wrought iron and steels.

The foreign metals such as manganese, &c., and other chemical impurities indicated by the analyses as present in the steels, &c., would doubtless have an important influence on these reactions, and the various oxides and gases accumulating round the metals from galvanic action would also tend to produce considerable effects.

It should also be borne in mind that steel is a complex crystalline product, and it would probably therefore on cooling, crystallise out in a somewhat varied manner, starting from the external surface from numerous nuclei. Such circumstances would naturally tend to reduce the perfect homogeneity of the metal as a whole.

These galvanometer-experiments appear to indicate that consolidation in the case of the steels and metals experimented upon had probably taken place in irregularly dispersed crystalline layers or delicate formations varying in molecular structure and composition from the surface to the centre. The sea-water gradually penetrating by corrosive effect, commencing to act on an inner portion of the metal of slightly different molecular crystalline structure, doubtless helped to produce the alterations in the electro-chemical positions observed. Iron being less crystalline and more fibrous would not appear to be so liable to exhibit such a tendency to this action as steel.

Apart from other causes the equilibrium is also further altered by the positive metal being more rapidly oxidised and acted upon than the negative metal; this in turn appears to assume the positive position, and thus the alternate variation is prolonged after the manner of a pendulum.

In conclusion, it may therefore generally be assumed that these reactions afford one basis for a correct indication of the extent of the corrosive action of sea-water on the different metals in combination, such results being dependent on causes similar to those herein suggested. The Author's wish has been to bring before the Institution in this Paper a number of interesting and perhaps novel facts relating to the corrosion of iron, steel, and cast-metal in connection in sea-water, and to treat the subject from one of its galvanic aspects. A clear and comprehensive understanding of the difficult question of corrosion, like other matters, can only be obtained by regarding it from many points of view, and by bringing a steady accumulation of facts to bear on such an intricate and knotty subject.[1]

The Paper is illustrated by diagrams and tables, from which Plate 8 has been prepared.

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[1] For information on the relative electro-chemical position of wrought-iron, steels, and cast metals in various waters and solutions, reference may be made to the Transactions of the Royal Society of Edinburgh, vol. xxxii., part 1.

APPENDIX.

Plate 8 represents curves of the galvanic action between the metals concerted in sea-water and changes in the electro-chemical position of the metals. The following notes relate to the five sets of curves:—

I. Wrought-iron bar (bright), connected with "soft" Bessemer steel bar (bright)——(1). Wrought-iron bar (bright), connected with "hard" Bessemer steel bar (bright)——(2).

(1) After the two-hundredth day measurements were taken daily until the completion of three hundred days, the electromotive force slowly decreasing to 0·0004 volt, the "soft" Bessemer steel continuing positive.

(2) Observations were taken daily till three hundred days were completed. During the last one hundred days the highest electromotive force recorded was 0·002 volt, and the lowest 0·0004 volt. In this period nine interchanges of electro-chemical position occurred.

II. Wrought-iron bar (bright), connected with "soft" Siemens-Martin steel bar (bright)——(1). Wrought-iron bar (bright), connected with "hard" Siemens-Martin steel bar (bright)——(2).

(1) After the two hundred and thirtieth day measurements were taken daily until the completion of the three-hundredth day, the result showing a gradual decrease of electromotive force to 0·0044 volt, the "soft" Siemens-Martin steel continuing metal positive to the end.

(2) After the two hundred and thirtieth day the electromotive force was daily measured until the completion of the three hundred days, remaining at 0·0004 volt until the two hundred and sixty-third day, from which time the electromotive force rose to 0·001 volt and continued thus until the end, the "hard" Siemens-Martin steel remaining metal positive.

III. Wrought-iron bar (bright), connected with "soft" cast-steel bar (bright) ——(1). Wrought-iron bar (bright), connected with "hard" cast-steel bar (bright)——(2).

(1) From the two hundred and twenty-fifth day daily measurements were taken to the end of three hundred days, the result being a steady decrease of electromotive force to 0·003 volt, the "soft" cast-steel retaining its position as metal positive.

(2) From the two hundred and twenty-fifth day daily measurements of the electromotive force were taken to the end of the three hundred days, a decrease of electromotive force to 0·004 volt being observed on the two hundred and seventy-fifth day, from whence it rose to 0·007 volt, the "hard" cast steel remaining metal positive.

IV. Wrought-iron bar (bright) connected with best cast-metal bar No. 1 (bright)——(1). Wrought-iron bar (bright) connected with common cast-metal bar No. 2 (bright)——(2).

(1) Measurements taken daily after the two hundred and sixtieth day to the end of the three hundred days, the result being a decrease of electromotive force to 0·001 volt, the best cast metal continuing metal negative.

(2) Daily measurements of the electromotive force were taken after the two hundred and sixtieth day to the end of the three hundred days, a steady

decrease of electromotive force to 0·001 volt being recorded, the common cast metal remaining metal negative.

V. Wrought Lowmoor-iron bar (bright) connected with "soft" Siemens-Martin steel bar (bright)——(1). Wrought-iron bar (bright) connected with Tungsten steel bar (bright)——(2).

(2) The Tungsten steel remained in connection with the wrought-iron bar for a total period of seventy-eight weeks. At the end of sixty-three weeks the electromotive force was 0·004 volt, the Tungsten steel being metal negative. At the end of seventy-eight weeks the electromotive force was 0·010 volt, the Tungsten steel being metal negative.

The Tungsten and wrought-iron bar in this set were 7·5 millimetres in diameter, 12 inches long, each bar immersed in sea-water to a depth of 8 inches, ½ inch apart.

(*Paper No. 2012*).

"The Comparative Value of Labour in different Countries."

By CHARLES ORMSBY BURGE, M. Inst. C.E.

THE following notes as regards manual work done, are chiefly the result of the Author's experience, as acting for direct employers of labour in various countries abroad and at home, including work by Englishmen, Frenchmen, Belgians, Italians, Greeks, Spaniards, Hottentots, Kaffirs, Zulus, Madrassees, Moplahs, Candaharees, and other tribes, varying as much in their daily rates of wages as from 3*d.* up to 10*s.* for the same character of work.

The late Mr. Brassey is said to have stated that, when native labour is fairly plentiful, difference of wages does not materially affect price of work, and no doubt, if the absence of labour-saving appliances, and the greater cost of transport and European superintendence, be set against the lower wages, the total cost of a large work abroad may not be greatly different from that of a similar one at home; but if he meant that wages generally adjust themselves to the ability of the workman, the statement is certainly open to question.

It is from the latter point of view that the subject is dealt with in this Paper, that class of work in which manual labour is the chief ingredient being alone considered in the comparison, and the result arrived at is, in general this, that the lower the wage is, the lower is the price of work; though of course, not in the same proportion.

To illustrate this, a few Tables are subjoined, showing roughly the comparison between the physical capabilities of various races for different classes of work, and that between the actual cost of the results produced. In every case the latter is obtained by dividing the sum paid direct to the pieceworker, by the number of units of work executed for it, and an average is taken from large totals:—

TABLE No. I.—EARTHWORK.

Side Cutting excavated and removed to Embankment. Nature of Soil, Lift, and Lead being approximately the same in each case.

—	Amount of Work done in a given time, average English Quantity being Unity.	Cost of Unit of Work, (Labour only), English average being Unity.
Englishmen	1·00	1·00
Frenchmen, Belgians, Germans, North Italians, &c	0·75 to 0·90	0·90 to 1·00
Southern Europeans	0·60 „ 0·85	0·60 „ 0·80
Hottentot half-breeds	0·50 „ 0·80	0·90 „ 1·25
Kaffirs, Fingoes, and Zulus	0·40 „ 0·70	0·80 „ 1·00
The stronger Indian races	0·40 „ 0·70	0·25 „ 0·60
Inferior „ „	0·25 „ 0·40	0·20 „ 0·50

TABLE II.—ORDINARY BRICKLAYING.

—	Amount of Work done in a given time, average English Quantity being Unity.	Cost of Unit of Work (skilled Labour only), English average being Unity.
Englishmen	1·00	1·00
Frenchmen, Germans, Dutchmen, &c.	0·80 to 1·00	0·80 to 1·00
Natives of India, best class	0·40 „ 0·50	0·50 „ 0·65
„ „ inferior	0·30 „ 0·40	0·43 „ 0·60

TABLE III.—HAMMER-SQUARED RUBBLE. DRESSING and SETTING ONLY.

—	Amount of Work done in a given time, average English Quantity being Unity.	Cost of Unit of Work (skilled Labour only), English average being Unity.
Englishmen	1·00	1·00
Frenchmen, Germans, &c.	0·75 to 0·90	0·90 to 1·00
Southern Europeans	0·55 „ 0·70	0·70 „ 0·80
Natives of India, best class	0·40 „ 0·60	0·65 „ 0·80
„ „ inferior	0·33 „ 0·50	0·60 „ 0·80

TABLE III.—PAINTING on NEW WORK.

—	Amount of Work done in a given time, average English Quantity being Unity.	Cost of Unit of Work (skilled Labour only), English average being Unity.
Englishmen	1·00	1·00
Natives of India	0·40 to 0·60	0·60 to 0·80

The skilled work, generally speaking, of the Englishman, it must be admitted, is superior to that of the foreigner.

Now the chief characteristic of these Tables is the difference in cost of result of unskilled abour in England and in India, and it is clearly shown that, to explain it, other causes must be brought than those produced by the ordinary supply and demand theory, which, if fully operating, would absolutely equalize the amounts paid for the same work everywhere.

Economists state that rates of wages for unskilled labour may be divided into two categories, namely, first the *market* rate, which is that which chiefly prevails in the more wealthy communities, and is mainly influenced by the laws of supply and demand; and secondly, the *natural* rate, where these laws find their limit, and which represents—having regard to the prices of commodities—the lowest means of sustenance for the workman and his family. It is obvious that no supply of labour, however great, can bring wages below this point; as should they be lower, the labourer must starve, and the work must come to an end. Now from various causes, which cannot be entered upon here, the natives of India and many other populous tropical countries, are to a great extent living on *natural* wages; and this explains partly the advantage which the Indian employer possesses over the English one, as shown by the Tables, in the cases considered.

Then there is in India the disinclination of the native to leave his village, and follow the demand for labour, and the absence of any ambition on his part to be any richer than his forefathers or neighbours; moreover, his domestic requirements are so few that the women are as numerously employed as the men, and this again tends to cheapen prices, the women being direct producers of work, while in Europe they are entirely occupied in providing for the necessities and comforts of their husbands or brothers, two people having to be fed for the work of one.

Again, the wages of the Indian native are nearly all returned to his employer in work done. They are mainly spent on food which, if not of a very strength-giving character, is cheap, and consists almost wholly of tissue-forming matter, all utilized in the work given in exchange for it, small as that work relatively is. Turning to the Englishman, beer, no doubt, supplies strength; but most people would admit that the three or four quarts drunk daily by the average British navvy, is more than is necessary for the work he has to perform. The tropical native commonly spends much of his spare time in sleep, thus storing up tissue to be expended for the benefit of his employer, and saving his money the while;[1] the white man, on the contrary, probably using the same opportunity for spending his substance in more exciting distractions, legitimate or otherwise, to provide for which the general rate of wages is kept up, and the equivalent return for it, in work, kept down.

The extreme case of European as compared with Indian unskilled labour, has been chiefly dwelt upon as showing, in a more emphatic way, the causes of divergences such as are shown in the Tables; but those of intermediate races, and of the artizans work might be similarly explained.

Another reason for divergence, which can hardly be left out of sight, is the method of working in different countries. As regards earthwork for instance, there are in England and other places, the "getter" excavating earth with a pick, the "filler" transferring it with a shovel to the barrow, which the "wheeler" drives to its destination on a wheeling-plank. In Spain, a donkey with a *seron* or pannier, and a boy to drive it, replaces the barrow and wheeler. In India the filler uses a tool something like a large hoe, with a short handle, called a *momati* or *phoura*, by which he fills a basket placed on the ground between his feet; he then assists a woman cooly to place it on her head, and it is carried in that fashion to the bank.[2]

Whether the mechanical advantage in the locomotive part of these operations lies in conveying the work through the arms to the spine as in the English method, or using the spine only, as in the Indian fashion, or throwing it entirely on to the donkey as in Spain, is a moot point. Comparing the use of the basket in India as against the barrow in England: a cooly woman at 3*d*. per day, will take sixty journeys to deposit a cubic yard of ordinary light clay, while an English navvy, at sixteen times the wages, will have to make 160 journeys with a barrow holding $\frac{1}{10}$ cubic yard, in the same time, to give the same value in work for the money paid to him for that time. In other words, allowing $1\frac{1}{2}$ mile per hour for the average walking speed of the woman, the navvy would have to drive his barrow at four miles per hour to compete with her.

The case of prices in sparsely peopled new countries, would occupy a wider field of inquiry than is advisable to undertake in the present Paper, involving questions of the enhanced rates due to imported labour and other considerations. Enough has been said to lead to the conclusion that every engineer in estimating, or contractor in tendering, for work abroad, unless guided by existing rates, should exercise great care in ascertaining the habits and character, as well as the rates of wages, of the people by whom any proposed works are to be constructed.

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[1] It is not unusual for the cooly to stop work before the end of the week, if he has earned enough to barely sustain life till the beginning of the following one, spending the interval in almost total inactivity and in sleep.

[2] See the Paper "Earthwork in Scinde," by W. H. Price, M. Inst. C.E., in "Professional Papers on Indian Engineering," January 1874, for comparative cost of barrow and basket work.

(*Paper No. 2014.*)

"Water-Supply in some parts of Peru; and Probyn's Distilling Apparatus at Iquique."

By Charles Malcolm Johnson, R.N., Assoc. M. Inst. C.E.

On the Pacific slope of the Andes, water for domestic purposes is a sufficiently scarce commodity—in certain districts—to offer a fascinating subject for the display of engineering skill and resource in its production; as well as promising large rewards to the successful practitioner. In not a few towns on the coasts of Peru and Chili, between Guayaquil in the north and Talcahuano in the south, there is an entire absence of springs, rivers, wells, or lakes, and boring—even when depths of hundreds of feet have been reached—has too frequently failed to strike the hidden treasure.

Rain is practically unknown on the Pacific slope, and the few showers which do fall in the course of a year, are not sufficiently heavy to lay even a portion of the dust. In consequence, the inhabitants are entirely dependent for water on the streams which come down from the Andes, the melted snows from the lofty sierras of the Cordilleras. But there are places on the coast and in the interior where no such streams are to be found.

Under the mild and paternal government of the Incas, every town and hamlet in Peru was well supplied with water. Barbarians though they were—or rather as they were considered by their Spanish conquerors—the remains of their aqueducts, water-courses, and irrigation works, show them to have been well acquainted with the primary laws of hydrostatics. But with the subjugation of the country by the Spaniards, the whole system of water-supply seems to have fallen into irreparable ruin. Yet such was the perfection of their workmanship, that some places in the interior of Peru are to this day provided with water by aqueducts and water-courses, whose direction is unknown, and whose sources are hidden mysteries; and the supply has never failed in the centuries which have passed since the Conquest. Unfortunately, towns and hamlets so supplied are few in number. The majority have to depend entirely upon the snow-water from the Andes.

At Iquique and at some other places in Peru, where during the present century large mining and other industries have rapidly developed, and where in consequence the growth of the population has been correspondingly rapid, all attempts to find water by boring have hitherto been unsuccessful. They are, therefore, dependent upon tank-vessels carrying water from other places on the coast, or upon systems of distillation, by which good wholesome water may be abstracted from the ocean. The want of one of the two great necessaries of life has naturally been prolific of ingenious contrivances for its production. Among the many, there is one which, from its simplicity and originality, appears worthy of mention.[1] It consists of a series of shallow wooden boxes, with sloping glass roofs, placed side by side on the sandy soil, and connected by pipes. Salt water is admitted into these boxes to a certain depth, and evaporation is promoted by the sun's rays passing through the glass roofs, which are placed in such a position as to absorb the maximum power of the sun's heat during visibility. The lower edge of the sloping glass terminates within the box, and immediately over an iron trough, into which the drops of water—distilled on the under surface of the roof—fall, and are conveyed to the receiving tank. The water thus distilled is exceptionally pure and wholesome; but the process is somewhat slow, and is occasionally retarded—though never entirely stopped during daylight—by the intervention of clouds. This process, however, has the great advantages of cheapness and cleanliness; but the large area, required for plant to produce any considerable quantity of water, has presumably militated against any extensive adoption of the method.

The town of Iquique is situated in Lat. 20° 12′ S., and Long. 70° 11′ W., on a narrow strip of sand at the foot of the Lower Andes. It is the southernmost port of the Peruvian Republic. At present it is in the hands of the Chilians, ostensibly held as security for the war-indemnity. No one, however, except, perhaps, a sanguine Peruvian, ever expects to see that flag float again from the government buildings. Iquique is the centre of the great nitrate beds, which extend north, south, and east of it; and although taxation has well nigh made the trade unprofitable, and the war has almost put an end to the employment of native labour, yet the major portion of the export continues to pass through Iquique, and will continue to do so as long as the trade exists. When the port was visited by the Author in October 1882, there were over one hundred vessels at anchor in the roadstead—harbour there is none. At the end of March 1884, there was not more than a fourth of that number there, and the prospects of the trade were by no means reassuring. Five years ago it was making rapid strides towards becoming a big city, and it may even yet be so, if the prohibitive taxes are reduced, allowing the nitrate and other industries to be developed to their full capacity. Iquique has no natural internal water-supply. At present its population is probably about 8,000, and the artificial supply of water for such a number of people, as well as for the vessels loading in the port, must always be a subject of serious importance. Until the opening of Mr. Probyn's works in July 1883, Iquique had been dependent for the supply of water upon tank-vessels and distilling apparatus of more or less primitive construction. With the present apparatus, from 40,000 to 50,000 gallons of water can be supplied daily; and when the new plant now being erected is in working order, it will be possible to double that quantity. Iquique will then be independent of external sources, as long as she has plenty of coal.[1]

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[1] This system was described in a Paper on "Apparatus for Solar Distillation," by J. Harding. Minutes of Proceedings Inst. C.E., vol. lxxiii., p. 284.

Probyn's apparatus consists of a Roots multitubular boiler, five generators, and a surface condenser. The Roots boiler is worked at a pressure of 140 lbs. per square inch. The water contained in it is worked over and over again, never being changed unless a leaky tube or other defect necessitates "blowing out." In such a case the boiler, when repaired, is refilled with distilled water, and the process of alternate evaporation and distillation proceeds as before. No zinc slabs are used for the protection of the tubes, nor is any deposit formed on their interior surfaces, yet they are singularly free from pitting or oxidation. This boiler, however, has only been in use about twelve months; but the foreman in charge stated that he had worked a similar boiler for fifteen years, in which the deterioration was scarcely perceptible. The steam generated in the Roots boiler passes into the first generator, which is placed at such an elevation that the condensed steam gravitates to the lowest part of the Roots boiler, and so maintains a constant water-level. All losses from leakage are made up by supplies drawn from the "distilled-water" pipes, on the last generator just before passing into the surface condenser, so that the least possible quantity of air is introduced into the boiler with the auxiliary feed. A small donkey-engine is attached to the boiler for this purpose, and for feeding the boiler from the first generator, should there be any sluggishness of circulation.

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[1] Formerly water for the locomotive-engines and for the other services of the Nitrate Railways of Iquique was obtained by distillation of sea-water by plant of the ordinary description, namely boilers generating steam to be passed through pipes immersed in cold-water tanks. At present the use of distilled water has been discontinued, as it is found to be cheaper to buy "Arica water" brought down the coast by steamers. This, moreover, has the advantage of suiting the engines better, as the distilled water caused them to prime.—SEC. INST. C.E.

The generators are all of one size and pattern, and all corresponding parts are interchangeable. Each generator is a cylindrical boiler, 20 feet long and 5 feet 6 inches in diameter; having two nests of wrought-iron tubes passing through it, one on each side of the middle line of the boiler. Through these the steam from the boiler, or the preceding generator passes (on its way back to the boiler, or on to the distilling apparatus, as the case may be, according to its position in the system), generating steam in its turn from the sea-water contained in the generator. The steam thus produced passes into the tubes of the next generator, and in its turn raises steam of a lower pressure; and so the process goes on to the end.

The pressure in the Roots boiler—as has been stated—is maintained at 140 lbs. In the first generator it is about 80 lbs.; in the second, 60 lbs.; in the third, 40 lbs.; in the fourth, 20 lbs.; and in the last about 10 lbs. per square inch. From all the generators the partially condensed steam passes through the surface condenser into the receiving tank at the rate of from 200 to 250 tons per day of twenty-four hours.

The density at which the generators are worked is about 30° (three times the density of sea-water); and this limit is maintained by the ordinary process of brining. A portion of the heat is abstracted from the brine by causing it to circulate about the feed-pipes to the generators, on its passage back to the sea. The generators and all steam-pipes are carefully clothed to prevent loss of heat by radiation; and all fresh-water pipes are left unclothed to promote condensation. Drain-pipes and catch-traps are used in all positions where water is likely to accumulate in the bends and elbows of steam-pipes, and to all cocks from which fresh water is blown when opened, so that the loss of fresh water may be reduced to a minimum. Any generator can be thrown out of work, for cleansing or repairs, at any time, without interfering with the process of distillation; the only effect being a reduction of the quantity of water produced. This is found to be necessary about every eight or nine days, to allow the scale to be removed from the tubes, and the sludge from the bottom of the generator. Some scale which was being removed from one of the generators, after it had been at work nine days without stopping, varied in thickness from $\frac{1}{4}$ inch at that end of the tubes where the steam entered, to about $\frac{1}{16}$ inch at the "exit" end. There were no laminations in the scale, but it had the appearance of a steady and continuous deposit; due, of course, to the fact that the process is never stopped, except in case of damage to the primary boiler, and still more to the uniform temperature maintained in the generators, by heating them with high-pressure steam, as contrasted with the intermittent and varying temperature maintained by ordinary stoking. Only two or three stoppages had occurred during the time the works had been in operation—nearly twelve months. One of these, due to a defective tube, lasted about three hours, another two and a half hours, but on neither occasion had the process of distillation altogether ceased, before steam was again raised in the primary boiler.

The foreman in charge stated, but time did not allow of this statement being verified, that they were then making 25 tons of water per ton of Lota coal; but that with good Welsh steam-coal, the efficiency of the apparatus would be as high as 40 tons of water per ton of coal. Lota coal is an inferior quality of bituminous coal, very fiery and smoky. It is procured from mines in Lota, in the south of Chili. The value assigned to it as a steam-producer, by engineers who have used it, in comparison with good Welsh coal is probably about as two to three.

OBITUARY NOTICES.

WALTER FRANCIS MONTAGU DOUGLAS SCOTT, fifth Duke of Buccleuch, and seventh Duke of Queensberry, K.G., P.C., D.C.L., LL.D., F.R.S., was born on the 25th of November, 1806. He died, full of years and honours, and universally respected, on the 16th of April, 1884. The story of his life is that of a great noble who, thoroughly imbued with the idea that wealth and position have their responsibilities as well as their privileges, conscientiously devoted himself during the whole of a long career to the good management of those estates from which he derived his princely resources. In realizing this view of his duties, far-seeing enterprise induced him to engage, at his own cost, in great works of engineering, the successful achievement of which constituted him a public benefactor. It is in connection with these works that the late Duke was made an honorary member of the Institution on the 28th of June, 1842, and the present memoir will therefore bear more particularly upon the circumstances which induced their carrying out.

The Duke of Buccleuch's ideas of the duties of a great landowner may be gathered from the following extract from a speech he made at what was long known as the Branxholme Fête of 1839:—"What has been entrusted to me has not been given that it might be wasted in idle or frivolous amusements; nor would I be justified in wasting the hard earnings of the tillers of the soil by carrying them away and spending them in foreign countries, but I wish to see them employed as the means of producing good to them and to the country at large." Acting up to this his attention was early directed to the establishment of a harbour at Granton. The lands of Wester Granton became an adjunct to the estates of the House of Buccleuch through the marriage of Lady Caroline Campbell, eldest daughter of John Duke of Argyll and Greenwich into the family. At the beginning of the present century, great inconvenience was felt by those interested in navigation through the want of a harbour on the east coast which could be entered by vessels at all states of the tide. At that time the slow and always dangerous method of landing passengers was by transferring them to boats at low water. The suggestion to erect a pier which would allow the largest vessels to be run alongside, at once commended itself to the favourable consideration of the Duke, who obtained a report from Messrs. Robert Stevenson and Son in 1834. This thoroughly satisfied him as to the feasibility and utility of the proposed works, and he lost no time in commencing operations. The gigantic undertaking was begun in 1835, and from that time onwards it is estimated that half a million sterling has been expended in connection with the harbour. Attention was first principally confined to the pier, which was so far constructed as to admit of its being partly opened, amid considerable festivities, on the day of the Queen's Coronation, in June 1838. But it was not till 1844 that it was finished to its present extent of 1,700 feet long by 180 feet broad. Before the final completion of the pier it was made use of for the landing of the Queen and the Prince Consort on the occasion of their first visit to Scotland, in September 1842, when the royal party was received by the Duke of Buccleuch and Sir Robert Peel. The point at which the Queen and the Prince Consort landed is at the northern end of the pier, and was sometime known as Victoria Jetty. The pier, the foundation of which was of stone from the neighbouring quarries, was constructed so as to have a depth of 10 feet at its outer end at low water, and 28 feet at high water spring tides. No expense was spared by the Duke in furnishing the pier with all the requisites in the way of warehouses, jetties, slips, coal-tipping apparatus, and cranes necessary to make it complete. The pier is protected by large breakwaters, for the construction of which an Act of Parliament was obtained in 1842. The portion of the Forth within which he had control of the shipping extends from Wardie Burn mouth on the east to the march of the lands of Granton and Muirhouse on the west, and it runs out for a considerable distance between these points into the estuary. The breakwaters enclose an area of 130 imperial acres, 77 of which are on the west side of the pier, and 53 on the east. The western one was first proceeded with. Its total length is about 3,100 feet, or nearly three-fifths of a mile; its breadth at the level of high tide is 24 feet, while at the base some portions are as much as 140 feet. Within the last few years this breakwater has been formed into a mineral wharf, with wide jetties for the accommodation of vessels coming to Granton to discharge grain and timber and take in coal. The eastern breakwater is 25 feet broad at the top, and 150 feet at the deepest part of the base. The width of the breakwater is 72 feet. Extensive dredging-apparatus was employed for the harbour, and the berths are being constantly deepened, so that in many of them vessels of large size lie afloat at low water of neap tides. The Granton Road, which has been so advantageous to the district, was constructed at the Duke's expense in order to give access to the harbour. It may fairly be stated that the port of Granton virtually owes its existence to the improvements thus made by the late Duke, and its inhabitants took many opportunities of showing their gratitude to him.

In all places where he had property, the Duke was forward in works of a public character, but mention should also be made of Barrow-in-Furness. He was Lord of the Liberty and Manors of Furness, and owner of extensive iron-mines. For the purpose of their better development he, in conjunction with the Duke of Devonshire, promoted, and chiefly at their own cost, constructed the original Furness Railway in 1843–44. This has been gradually developed into a coast-line of the first importance, and large docks have been built. The Duke of Buccleuch continued to be a director of the Furness Railway until his death, and always took an active interest in the progress of the company, and especially in the Barrow Docks. It may be mentioned, in passing, that Furness-Abbey railway station has been so built as to avoid, as far as possible, any flagrant violation of the *genius loci*, and that the noble ruins adjacent may be enjoyed by the public without the eye being offended by any harsh contrast between ancient beauty and modern utility. He did not inherit much landed estate in Furness, but of late years he purchased largely, and at the time of his death practically owned the division of Lindal and Martin, in the parish of Dalton in Furness. He rebuilt the village of Lindal, converting it from a collection of hovels into a "model village," with all sanitary and other modern appliances. He also built new farmhouses and outbuildings at most of his farms, and had he lived another two years, would have completed his improvements and made the estate as nearly perfect as such things can be.

The record of the Duke of Buccleuch's great work at Granton, and his improvements at Barrow, have been thus noticed, as they constitute the instances in which his ideas were carried out on the largest scale; but the same system was adopted, in unstinted fulness, on all his other estates.

In this connection an incident, which took place at a meeting during the agitation against the law of hypothec some twenty years ago, is worthy of being narrated. A farmer made a speech in which he pointed out the disadvantages experienced by his class under the existing law. A gentleman, who evidently knew the farmer's circumstances, said to him, "But you do not suffer from the grievances to which you refer?" And the farmer replied, "The land I rent is too near that owned by the Duke of Buccleuch for me to suffer from them." So powerful was the practice and influence of the late Duke on neighbouring landowners.

He was an eminently practical man. He brought his great personal experience to bear on all the details necessary to ensure the successful carrying out of the various improvements on his estates, whether as regarded agriculture or building. He went to his office and transacted business much the same as a city merchant would do, and always kept in hand the personal direction of what was going on.

Of the Duke of Buccleuch's public life it is not necessary here to speak. It may suffice to say that the same generosity of character he displayed in private matters he extended to his political connections, whether rival or friendly. Though a staunch Conservative, his political opponents were among the foremost to appreciate him, and no higher tribute could be paid to him than the eloquent fact of the present Prime Minister, Mr. Gladstone, being one of the first to subscribe to the national testimonial to the Duke, set on foot some years ago, to show the estimation in which he was universally held. His unanimous election to the Chancellorship of Glasgow University, in 1878, further testified how far personal esteem for him rose above political jealousies. He was President of the British Association at its meeting in Dundee in 1867, and was fellow or honorary member of many other societies, especially those established to encourage the advancement of agriculture.

The life thus usefully spent for the benefit of others was the successful effort of a good man to prove that the "bloated landowner" is not necessarily an enemy of the people.

LESLIE CLARK was born and educated at Aberdeen, of which city his father was for some years the Provost.

In 1861 he was articled to Mr. Abernethy, Past President Inst. C.E., with whom he remained as a pupil until 1864. After being for a short time assistant engineer on the Ceylon Railways, which were in charge of Mr. Molesworth, M. Inst. C.E., he was employed on the docks and drainage-works in Bombay. He was also for several years in the service of the Bombay, Baroda, and Central India and the Great Indian Peninsula Railway

Companies, during which period he was a district engineer, engaged upon the construction of the Wardha and Wadwan lines. In 1875, he was appointed by the municipality of Lahore, to design and execute a system of waterworks and of drainage for that city. His proposals, after a lengthened discussion, received the approval of Government in the year 1877; but owing to the want of funds, only a portion of the drainage-scheme was executed. The waterworks-scheme, consisting of a system of wells sunk in the low lands near the river-bed, from which the water was pumped to a high-service masonry reservoir in the city, and thence distributed along the main streets by iron piping, was completed in 1881, and ready for opening. Unfortunately, however, the portion of the drainage-scheme which was intended as an outfall for the reservoir, was not sanctioned by the Punjab government. The reservoir was filled experimentally; but in passing off the water into some disused wells near the reservoir (which were used after consultation with the government engineers), it permeated the adjoining ground, and caused a subsidence in the foundations, which resulted in the destruction of the reservoir. Upon an official inquiry into the cause of the disaster, Mr. Clark was absolved from all blame, and subsequently received orders to design and erect a new reservoir.

As soon as the disaster happened, Mr. Clark took immediate steps to utilize a high-pressure system, which had been constructed for use in case of fire, and by pumping over this service, was enabled to give the city an intermittent supply, pending the completion of the new reservoir. This system has worked admirably, and on the 7th of April, 1884, the Lieutenant-Governor of the Punjab formally opened the new iron reservoir, which had been designed by Mr. Clark, and was in course of erection at the date of his death.

Mr. Clark was also under contract with the Punjab government to design a system of drainage and waterworks for the city of Amritsar, which he had unfortunately not quite completed. Besides this, he was in treaty with the municipality of Delhi for a similar service. He was much respected for his ability. The Lahore waterworks being the first of the kind in the Punjab, will be a standing monument of his engineering skill.

Mr. Clark was a man of great business-capacity; but possessed an unhappy irascibility of temper, which occasionally acted to his detriment. He was singularly unfortunate in the failure of the first reservoir, from causes over which he had no control, and it is much to be regretted that his death should have occurred at a time when he had just established his reputation as a waterworks engineer, and when his services were coming so much into request for the execution of municipal projects that he would have been able to have obtained pecuniary consolation for his previous misfortunes.

The cause of Mr. Clark's death was abscess of the liver, on account of which he was ordered to England in April, 1883, and after being operated upon by the most skilful surgeons in London, he succumbed to the disease at Broadstairs, in Kent, on the 27th of August, 1883, at the comparatively early age of fifty-five years. He was elected a Member on the 3rd of August, 1878.

Sir WILLIAM SIEMENS,[1] F.R.S., D.C.L., LL.D., Member of Council, died, after a very short illness, on the 19th of November, 1883.

Perhaps on no former occasion has the Institution had to mourn the loss of a member so distinguished for the extent of his scientific and technical knowledge, for the versatility of his talents, for the originality of his views, for the greatness and variety of his inventions, for his amazing skill in the adaptation of science to industrial and useful purposes, for the number and value of his scientific publications, for his energy in business transactions, and for the usefulness to the world of his life's work generally.

His father, Christian Ferdinand Siemens, was "Domainenpächter" of Lenthe, near Hanover, and married a lady of a well-known Hanoverian family, named Eleonore Deichmann. They had four daughters, and ten sons, three of whom, Werner, Carl, and Friedrich, had intimate business connections with the subject of this memoir during his whole career. The four brothers worked so harmoniously together that it is often difficult to say what was the precise personal share either of them had in the many great inventions for which the world is indebted to them, the task being rendered still harder by the fact that each brother was always ready to attribute a successful invention to any of the family rather than to himself. It may, however, be said that in electrical matters Werner and William were principally associated, while in the metallurgical inventions and furnaces Friedrich and William worked together.

The fourth son was born at Lenthe on the 4th of April, 1823. He was baptised as Carl Wilhelm, but as a younger brother (in accordance with a custom not uncommon in Germany) was also given the name of Carl, the elder one was generally designated by his second name only. After his naturalization in England, the German names became Charles William; but he always preferred the latter word used singly, and when he received his knighthood he elected to be called "Sir William" only.

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[1] For this Memoir the Institution is indebted to Dr. William Pole, F.R.S., Member of Council.—Sec. Inst. C.E.

For his preliminary education he was sent first to a school called the "Catherineum," at Lubeck, and then to the "Handelsschule" at Magdeburg. In his eighteenth year he went to the University of Göttingen, where he studied for a year or two under Dr. Wöhler, Professor of Natural Philosophy, and Herr Himly, Professor of Chemistry, who had shortly before married Fräulein Siemens, his sister. It appears to have been intended that he should devote himself to mechanical engineering, for on leaving Göttingen he was sent as a pupil to the works of Count Stolberg, a well-known manufacturer of engines and machinery at Magdeburg.

He manifested early that varied talent which was afterwards so characteristic of him, for his first prominent effort was the invention, in conjunction with his brother Werner, of an improvement in electro-gilding, then a new application of electricity. Believing that there would be a better market for this invention in England than in Germany, he visited this country in 1843, and succeeded in getting it adopted in Birmingham, "the toy-shop of the world," by the eminent manufacturing firm, Messrs. Elkington. The success of this visit emboldened him to return the following year with another invention, this time entirely his own, and of a more strictly engineering character, a "chronometric governor." He was less successful in the disposal of this, as it was somewhat in advance of practical wants, but his visit had the more important effect of determining his subsequent career. His efforts to introduce his invention brought him into contact with the leading engineers of this country, and he received so much encouragement from them that he was led to believe that a more congenial and more profitable field for his labours would be found in England than in his native country, and from that time England became his home.

His first years in this country were devoted partly to improving his mechanical and engineering knowledge, and partly to working out his own inventions, for which latter purpose he passed some time with the Birmingham firm who had at first taken him by the hand. During the railway mania he undertook some railway work, but he did not follow this up, as he conceived he could do better in other branches of mechanics. About this time he appears to have been engaged on several inventions, among them being the process of anastatic printing (on which a lecture was given by Faraday at the Royal Institution); some improvements in the cotton manufacture; and a new form of air-exhausting pump, which has since been much used.

But he soon turned his attention to matters of a higher character. He had an idea that great improvements might be made in the steam-engine, and to enable him to put his views into practical form, he went, about 1847, to the well known works of Messrs. Fox, Henderson & Co., of Smethwick, near Birmingham, where he remained four years. Here he carried on experiments which served as a preparation for the brilliant heat-applications of his after-life; and, what was of equal importance, his work and attentive observation in a factory renowned as one of the first schools of mechanical engineering in the country, gave him an amount of practical knowledge and experience which was of the greatest benefit to him.

In 1851 he went into practice on his own account, and after that date the chronicle of his career will be best indicated by a notice of the works on which he was from time to time engaged, and of the various matters which occupied his attention.

The private incidents of his subsequent life that need to be specially mentioned were few. In 1859 he became formally a naturalized subject of Queen Victoria, and in the same year he married Anne, daughter of Joseph Gordon, W.S., of Edinburgh, and sister to the late Lewis Gordon, F.R.S.E., well known as a civil engineer, and for many years Professor of Civil Engineering at the University of Glasgow.

In attempting now to give an account of the work of his mature career a difficulty is met with at the outset, owing to the varied nature of the subjects that occupied him. He had the extraordinary faculty of devoting his attention to many different lines of thought, and many different subjects of investigation, at one and the same period, for which reason, any attempt at a chronological arrangement would only be confusing. It will be better to classify his labours, and there are two subjects that may be taken as the chief headings in this notice. It may be said generally that the most important objects of his life were connected with the study of the two great sources of power in nature, Heat and Electricity, both in their theoretical aspects and in their practical and useful applications. Hence it will be well to treat separately of his work in regard to (I.) *Heat and its appli-*

cations, particularly to Metallurgy, and (II.) *Electrical Science and Practice;* to which may be added (III.) *Miscellaneous Engineering Mechanical and Scientific matters* not included under the former heads.

HEAT AND ITS APPLICATIONS, PARTICULARLY TO METALLURGY.

This may be said, without much risk of error, to be the most important subject of Sir William Siemens's study. He employed himself upon it, in some form or other, throughout the whole working period of his life, and the results of his labours in regard to it assumed a magnitude beyond that of any other of his occupations.

There is no doubt that in his early youth he had studied the theory of heat, and he kept pace with all the later discoveries in regard to it. He had made himself master of the profound investigations of Joule, Mayer, Carnot, and others, and had become well acquainted with the great modern doctrine of the conservation of energy, of which the dynamic theories of heat had furnished such conclusive demonstration. Applying to these theoretical considerations his eminently practical mind, he could not fail to see what an enormous loss of valuable energy was continually going on, by the waste of heat, in almost all manufacturing and industrial processes, and it became his earnest endeavour to discover and introduce means of economising this wasted power.

His first efforts were naturally directed to the machine which formed the chief practical source of heat-power, namely, the steam-engine, for it had been one of the first corollaries from the thermodynamic theory that this machine, in its most perfected state, only utilized a small fraction of the energy developed by its combustion of coal. He took this subject in hand, as already stated, soon after settling in England, namely about 1847; and it would appear that in his first efforts at heat-saving, he adopted and put in practice the eminently simple and practical idea which afterwards formed the leading feature of his great heat-inventions, namely, what he called the "regenerative principle." He found that in almost all industrial applications of fuel, much heat was lost by the passing away of currents at high temperatures, and it occurred to him that by presenting suitable masses of solid conducting matter to these currents their superfluous heat might be taken up, and might then be given out again in some useful way. It is right to say that the principle in question was not claimed as an original discovery by Mr. Siemens. It had been clearly described in a patent taken out as early as 1816 by the Rev. Dr. Stirling, and had been applied both by him and by Capt. Ericsson to heated-air engines, but it was looked upon by engineers as unsound in principle, and its application had very little beneficial result. Mr. Siemens saw not only its theoretical correctness, but its great practical value, and the credit of its wide success is undoubtedly due to him.

Believing that the chief loss of heat in the steam-engine was in the condenser, he endeavoured to apply the regenerative principle to this apparatus; and he constructed in 1847 an engine of four horse-power on this plan, which worked with some success at the factory of Messrs. Hick at Bolton. A few years more study and trial confirmed his general views on the subject, and in 1851 he wrote for the Institution of Mechanical Engineers a Paper "On a New Regenerative Condenser for High and Low Pressure Steam-Engines." This was his first literary effort, and was the pioneer of the long list of scientific publications he afterwards gave to the world.

It was followed in 1852 by another Paper laid before this Institution, on the same subject, but of a more general character, "On the Conversion of Heat into Mechanical Effect." This was an essay showing great knowledge and ability, both theoretical and practical, and it received from the Institution the award of the silver Telford Medal. In the same year, also, he undertook, for his own satisfaction, an important series of experiments "On the total heat of Steam, and its expansion when in an isolated state," which he described to the Institution of Mechanical Engineers.

The practical result of all this study was the design of a complete "regenerative steam-engine," which, from the promise it gave of a large saving of fuel, attracted much attention, particularly on the Continent, where companies were formed for its manufacture. Several engines on this principle were made, varying from five to forty horse-power, and two of them were shown and worked at the Paris Exhibition of 1855. The regenerator fulfilled its office with surprising rapidity and perfection, and considerable economy of fuel resulted. But practical difficulties were found to exist in the working of the engines, and notwithstanding the advantages they offered, they could not be brought into general use.

This disappointment, in an invention which had occupied ten or twelve of the best years of Mr. Siemens's life, would have disheartened most men, but it only brought out more forcibly his strength of character. He relied on the soundness of the theoretical principles that had guided him, and, having still full confidence in the feasibility of his plans for heat-saving, he searched for other modes of carrying them into effect. He soon found that a better result might be obtained by applying the regenerative principle in a more simple manner, *i.e.*, not to any form of complex engine, but directly to the ordinary furnaces in which fuel was consumed. It often happens, in the history of an invention, that the simplest form is the latest in presenting itself, and this furnishes a striking instance of the fact.

The invention of the regenerative furnace was due to Friedrich Siemens, but William worked heartily with him, and was the chief agent in maturing it and directing its various applications. It was patented in 1856, and was described in a paper read before the Institution of Mechanical Engineers in June 1857. The earliest experimental trials were conclusive as to the saving of fuel to be effected; but many formidable practical difficulties had to be overcome, and many improvements had to be introduced from time to time before full success was attained.

One of these improvements was of so important a character as to amount in fact to a new invention. It was found that the use of solid fuel, in the body of the furnace, offered obstacles to the favourable working of the system, and the idea arose of substituting gaseous fuel, the solid fuel being converted into combustible gases in a separate construction called a "gas-producer." This gave immensely increased power to the furnace, and made it applicable to much more extended processes, while its facility of management became much increased. It also allowed the use of inferior fuel, for combustible matters which would be utterly useless in an ordinary furnace were capable of being utilized without any difficulty in the new gas-producer.

The results of experience, during the quarter of a century that has elapsed since the introduction of this invention, have tended constantly to increase the estimation of its value, and to extend its use to a greater variety of purposes. Indeed there is hardly an industrial process of any kind where great heat is required, that has not benefited by the application of the Siemens furnace; the advantages being not only an enormous economy of fuel, but the production of a much higher temperature, greatly increased facility of application and management, more perfect combustion, and an almost entire absence of smoke. After nearly twenty years of continuous working and extended application, Sir Henry Bessemer, in 1880, described it as a beautiful invention, which was at once the most philosophic in principle, the most powerful in action, and the most economic of all the contrivances for producing heat by the combustion of fuel.

In the course of Mr. Siemens's labours in perfecting his regenerative gas-furnace, he had good opportunities of considering the various purposes to which it might be applied, and among others, it occurred to him that it might be beneficially used for the production of steel.

A suggestion had been made some time before, by a well-known steel maker, Heath, that cast-steel might be obtained in large quantities by simply fusing wrought- and cast-iron together on the open hearth of a reverberatory furnace. The experiment had been tried, but it had not hitherto succeeded, in consequence of the defects and want of power in the furnaces employed. Mr. Siemens saw that with the increased capabilities and the higher temperature afforded by his furnaces, it might be possible to carry out Heath's idea; indeed, it had been mentioned in the furnace-patent of 1861, and had been in that year suggested to one of the great Welsh ironmasters. In 1862 a trial was made at Tow Law, near Durham, and in succeeding years the experiments were renewed in France, in Glasgow, and at Barrow-in-Furness. They were successful in producing good steel, but difficulties were met with, and the manufacture was not commercially profitable. Under these circumstances Mr. Siemens perceived that it was necessary to take the matter into his own hands, and in 1865 he determined to erect experimental works in Birmingham for the purpose of maturing the details of his processes before inviting manufacturers to adopt them. These works, which were known as the "Siemens Sample-Steel Works," were in action during several years, and, although they were carried on under great disadvantages, as he had entirely to educate his own workmen, the results were considered so good as to warrant the extension of the process, and this led to the establishment of works of considerable magnitude at Landore, near Swansea.

But, Dr. Siemens was not the only one who had been turning his attention during those years to the manufacture of open-hearth steel. Messrs. Martin, of Sireuil, in France, had also been experimenting on the use of the Siemens furnace for the same purpose, but had chosen a somewhat different mode of operation, and thus originated the so-called "Siemens-Martin" process.

No sooner had the success of the new processes been proved than they were at once taken up by manufacturers on a large scale, and as early as 1869 some thousands of tons of first-class steel were made by them. Since then they have been very largely adopted, and the metal produced has been of excellent quality. In the year 1883, the production of steel made by these processes in the United Kingdom was nearly 500,000 tons, and for the whole world it may be estimated at about 1,000,000 tons.

Notwithstanding the great things Dr. Siemens had effected in the domain of heat, he did not disdain applications of his scientific knowledge on a smaller scale, having always in view economy and utility. He took an active part in the movement for smoke abatement, which was in agitation about the year 1880, and he applied his mind to several inventions for aiding in the suppression of the smoke-nuisance.

One of these was his domestic fire, now so well-known, in which, by inserting a few gas jets in a common grate, he rendered it possible to make a fire with ordinary coke, which, while absolutely smokeless, should be as cheap and as cheerful as that from raw coal, and much easier to regulate and to manage. This, although apparently so simple, was really a very scientific invention, for its principle was that of resupplying to the coke the hydrocarbons which had been abstracted from it, and so restoring the original elements of coal, but in an improved and refined form. Another scientific feature in the grate was that of feeding it, by a simple and ingenious arrangement, with heated air, so as to increase the activity of combustion. It was, in fact, his regenerative gas-furnace, brought into the drawing-room.

But he carried the same idea farther, for he believed that, by some simple modifications of his furnace, he could apply the gas-producing process on a small scale to ordinary steam-boilers and other applications of fuel. He had established experimental apparatus for this purpose, and only a few days before his death he wrote to a friend, expressing sanguine anticipations that the result would show the new process to be a considerable improvement on the ordinary plans of combustion, not only in smokelessness, but also in convenience and economy. He even went so far as to anticipate that the time might come when, by means of his gas-producer, gas might be made in the coal districts, and supplied by a gigantic system of distribution to the public generally as fuel. He told the people of Glasgow, in 1881, that that town, with its adjoining coal-field, appeared to be a particularly favourable locality for putting such a plan to a practical trial, and added that when thus supplied with gaseous fuel, the town would not only possess a clear atmosphere, but would be relieved of the most objectionable portion of the street traffic. A year later he declared, at the British Association, that he thought the time not far distant when both rich and poor would resort to gas as the most convenient, the cleanest, and the cheapest of heating agents, and when raw coal would be seen only at the collieries.

ELECTRICAL SCIENCE AND PRACTICE.

The science of electricity also engaged Mr. Siemens's attention when quite young. His first visit to England was on account of one of its practical applications, and in 1847 he made some important modifications in the Grove battery.

When he commenced business on his own account in 1851, his principal employment appears to have been with electric telegraphs. He had an office in John Street, Adelphi, and finding a want of sufficient means of carrying out his plans, he established a small factory for electrical apparatus at Millbank, which he retained for some years. Finding this answer well, and anticipating what a large field would soon open for the manufacture of electric apparatus, and what greatly increased facilities this would offer for his inventive powers, he determined to enlarge greatly this sphere of his labour. Accordingly in 1858, in association with his brother Werner, and a German friend, Herr Halske, he established the manufacturing works at Charlton, near Woolwich, since so well known under the Siemens name. These soon increased in size, as the applications of electricity became more varied and extended, and some of the largest works in electrical engineering have been done there. It must suffice here to mention briefly a few of the most important of them.

In 1867 the Messrs. Siemens obtained concessions for the Indo-European Telegraph, being an overland double line from England to India, through Prussia, Southern Russia, and Persia. The Prussian Government built the line through North Germany, and the Messrs. Siemens constructed the whole of the remainder, a distance of 2,700 miles. It was tendered for in the middle of 1868, and although the work involved great difficulties, passing through an unsettled and uncommercial country, yet by ingenuity and good management the difficulties were overcome, and the lines were completed by the end of 1869.

In 1874–5 the firm manufactured and laid the Direct United States Transatlantic Telegraph cable, from Ireland to Nova Scotia, and this work is noted as having given rise to the construction of a special steam-vessel for laying the cable. For the first Atlantic cables the "Great Eastern" and other vessels more or less suitable had been employed. But William Siemens, conceiving that the requirements of the operation could not be efficiently met by any existing type of vessel, resolved to design one specially for the purpose. The result was the production of a fine steam-ship, which, in compliment to the great electrical philosopher, was named the "Faraday." She performed the work required most successfully, has since laid other Atlantic cables, and is still used on work of the same class. She is 5000 tons register, 360 feet long, 52 feet beam, and 36 feet deep. In her interior are three enormous iron tanks for receiving 1,700 miles of cable, which are so ingeniously built into the body of the ship, as to add materially to the strength of her structure. She is completely fitted up with machinery of all kinds for laying the cable in the most efficient manner, as well as for grappling and recovering lost cables, and performing all accessory operations: many parts of her structure and apparatus show considerable novelty of design, among which may be mentioned a peculiar arrangement of the twin screw propeller, by which she can be manœuvred with great ease, and caused to turn even in her own length. She has good cabin accommodation, and is lighted by the electric light. The success of this ship, for the delicate and difficult purposes she is intended to serve, has been perfect, for she has proved herself capable of laying and lifting cables at all depths, in all seasons, and in almost all weathers; and it has been well remarked that the design of such a vessel, capable of doing what no other vessel afloat could do, by a landsman born in the interior of Europe, whose education and pursuits had little or no connection with nautical affairs, is a striking example of practical genius of the highest character.

One of the newest applications of electricity is the use of electric power for propulsion on railways. This was first brought about by Werner Siemens, who exhibited in 1879 a short tramway so worked, and afterwards laid down a line for public traffic at Lichterfelde, near Berlin, 2,500 yards long. A short line of the same kind was also laid down at Paris. A later and larger application was a line of 6½ miles long from Portrush to Bush Mills, in Ireland. The power was furnished by a waterfall of the river Bush, driving, through turbines, a Siemens dynamo, at a distance of 7½ miles from the line. The line had steep gradients and sharp curves, but a speed was attained on it of 10 miles an hour. It was formally opened by the Lord Lieutenant of Ireland, only two months before Sir William's death. It was never pretended that this mode of applying power could supersede the steam locomotive for general railway haulage, but the inventors looked forward to a considerable use for it in cases where exceptional circumstances might justify its application, and give it a preference.

It need hardly be said that Sir William gave great personal attention to the manufacturing works, and the result was that the multifarious processes there carried on, and the great variety of apparatus that issued therefrom, bore largely the impress of his originality and inventive power. It was a saying common in the workshops that as soon as any particular problem had been given up by everybody as a "bad job," it had only to be taken to Dr. Siemens for him to suggest half a dozen ways of solving it, two of which would be complicated and impracticable, two difficult, and two perfectly satisfactory.

Some of his personal electric work requires special mention.

In 1860 Werner and William had brought out the contrivance now known as the one-coil Siemens armature, which was shown at the International Exhibition of that year, and which appears to have formed the groundwork of a most important invention introduced a few years later, namely, the principle of electro-magnetic augmentation and maintenance of a current without the aid of steel or other permanent magnets. This was communicated to the Royal Society by William Siemens on February 14, 1867, in the now classical Paper "On the Conversion of Dynamical into Electrical force without the aid of Permanent Magnetism." The Author said :—

> "Since the great discovery of magnetic electricity by Faraday in 1830. electricians have had recourse to mechanical force for the production of their most powerful effects; but the power of the magneto-electrical machine seems to depend in an equal measure upon the force expended, on the one hand, and upon permanent magnetism on the other.
>
> "An experiment, however, has been lately suggested to me by my brother. Dr. Werner Siemens, of Berlin, which proves that permanent magnetism is not requisite in order to convert mechanical into electrical force; and the result obtained by this experiment is remarkable, not only because it demonstrates this hitherto unrecognised fact, but also because it provides a simple means of producing very powerful electrical effects."

It is singular that the same idea had occurred almost simultaneously to Wheatstone (who communicated it to the same meeting on the same day), and to Mr. Varley, who had applied for a patent in which it was embodied; but there can be no doubt of the *bona fides* of the Siemens's claim to an independent discovery.

Soon after followed the suggestion of the mode of connection between the coils of a multiple-coil Siemens armature, and this made the foundation of the wonderful dynamo-electric machines of the present day, whether as given in the Siemens form, or with the modifications of details and proportions which have been contributed by others. It has been well remarked by a high authority, that the evolution of the present splendid machine from the rudimentary armature of a quarter of a century ago, is one of the most beautiful products of inventive genius, and is more like to the growth of flower than to almost anything else in the way of mechanism made by man.[1]

William Siemens conducted many of his experimental researches on electricity in the retirement of his country seat at Sherwood, near Tunbridge Wells. Here he had a large and comprehensive electric "installation," and those who had the advantage of admission to that scientific retreat can best appreciate how much he did individually to reduce to human servitude the forces of that mysterious power of which he was so great a master. It was not only employed in lighting the premises, but also in supplying power to perform a large part of the work required on the estate, such as sawing wood, pumping water, and so on. It was here that he carried out the experimental investigation of a very original idea that had occurred to him, namely, that electric light might be made to supply the place of the sun in promoting vegetation, and in stimulating the growth of plants and fruits. The subject formed the basis of several published Papers, giving results of much interest and novelty.

Among the various applications of electricity proposed by him was one which, although it never had any extensive practical application in his time, was undoubtedly of great importance and capability, and will possibly some day be largely used, namely, as a means of transmitting mechanical power to a distance. In his address to the Iron and Steel Institute, 1877, he gave a striking example of what he thought might be done in this way. He pointed out the great amount of mechanical power developed by the Falls of Niagara, amounting to about 16,800,000 HP., and speculated on the probability of some portion of it being made useful by transmission to a distance, by electrical conductors. In a lecture to this Institution in March 1883, he again took up the subject, and instanced several cases where the transmission had been actually carried into practice.

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[1] Sir William Thomson, in *Nature*, Nov. 29, 1883.

Another use he conceived for electricity was to obtain great heat, for which purpose he contrived an electrical furnace, intended to melt on a large scale refractory substances which had hitherto been found only in minute beads, or obtained by enormous and costly erections.

He wrote, at various times, many Papers on electrical subjects, which show strikingly the originality of his views, and his great practical inventive power.

MISCELLANEOUS ENGINEERING, MECHANICAL, AND SCIENTIFIC MATTERS.

The versatility of Sir William Siemens's talents and powers has been already alluded to, and, large as was the field offered for his labours in each of the two great subjects already treated of, he contrived to find time and opportunity for attending to many other matters having little or no connexion with them. It remains to notice briefly some of his most important labours of this kind, whether in the form of mechanical inventions, or of published studies on scientific subjects.

Uniform rotation.—One of his earliest inventions, in fact the one which determined his permanent adoption of England as his home, was a contrivance for ensuring uniform velocity in steam-engines and other rotating prime-movers. The subject, however, seemed to have an attraction for him, and he returned to it some years later. In 1866 he read a Paper before the Royal Society on "Uniform Rotation," describing a new and very ingenious "Gyrometric Governor," by which the driving power might be varied between the widest limits, without any sensible variation of speed; and he proved by experiment that two-thirds of the total load on a steam-engine might be suddenly thrown off without any visible change in its velocity of rotation.

Water-Meter.—Another early invention was a machine for measuring water. This consisted only of a wheel enclosed in a tube through which flowed the water to be measured, the wheel being turned by the reaction of the water, and the velocity of rotation giving thus an indication of the velocity of the flow. But although the principle was so simple, its suitable application in a practical form needed great ingenuity, and its inventor had to bestow much study and experiment on it before its action could be relied on. It was patented in 1852, and improved a few years later. Its simplicity caused it to be promptly appreciated, and it

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furnished the inventor with his earliest source of income in England, so enabling him to give to this country the benefit of his great inventive powers in other matters. The "Siemens Water-meter" is now very largely used.

Vessels to resist high pressure.—In 1877, at the request of Colonel Beaumont, who was making large use of compressed air under high pressure as a source of power, Dr. Siemens designed a new kind of vessel, to be used as a reservoir or boiler, capable of resisting great internal pressure, and of remarkable lightness and strength. One was made with a capacity of 100 cubic feet, not exceeding 2½ tons in weight, and was perfectly tight at 1,300 lbs. pressure per square inch. He described it to the Institution of Mechanical Engineers in the following year, and gave it as an example of the superiority of steel for such purposes.

Pyrometer.—Another of his inventions was that of an improved Pyrometer, or Thermometer for very high temperatures. The exact measurement of great degrees of heat had always presented serious difficulties, the ordinary means being a very rough one, contrived by Wedgwood, and acting by the contraction of an argillaceous material. In the year 1860, having charge, for the Government, of the electrical condition of a submarine cable to be laid between Rangoon and Singapore, some observations led him to believe that variations of electrical resistance might be made use of to determine variations of temperature, and he addressed a letter to this effect to Dr. Tyndall, which was published in the "Philosophical Magazine" of January, 1861. Ten years later, namely, at the meeting of the Iron and Steel Institute at Merthyr, in September, 1870, he described the instrument itself as it might be applied to iron furnaces, and on 27th April, 1871, he read a Paper to the Royal Society giving a more full scientific account of it. He showed that in this way temperatures exceeding the welding-point of iron, and approaching the melting-point of platinum, could be measured by the same instrument by which slight variations of ordinary temperatures are told, a thermometer scale being thus obtained, embracing without a break the entire range. In a subsequent Paper, June 1882, he stated that the instrument had been actually and successfully used for showing the degree of heat in the interior of iron furnaces, and for ascertaining the temperature in distant and inaccessible places, such as the bottom of the ocean.

Bathometer and Attraction-Meter.—In the course of his submarine cable operations, he conceived that it might be possible to measure the depth of the sea on board a ship without sounding. The idea on which he founded this opinion was, that the total attractive force of the earth must be sensibly diminished by the interposition of a comparatively light substance such as sea water, between the vessel and the solid portion of the earth below, and that the degree of diminution would depend on the depth of the water. Hence, if an instrument could be constructed sensitive enough to show the diminution of gravity, this would indicate at the surface the depth of the sounding. He constructed such an instrument, and it was tried on board the "Faraday" with fair success.

He also contrived another instrument on an analogous principle, but for measuring horizontal attractions, which he called an "Attraction meter." It was so sensitive, that the fact of a person moving from one side of it to the other caused the indicator to move sensibly. It was also clearly influenced by the gravitation of the sun and moon in the heavens, the fluid in it being in fact subject to a perceptible daily tide.

The Sun.—One of the latest matters that occupied his mind was a speculation which, though of a highly scientific character, differed from his more usual subjects of thought in that it had no immediate practical bearing; it was no less a matter than the cosmical functions of the sun, and the nature of the solar energy.

His researches into light and heat had led him to think deeply about their great source in the solar system, and after much careful study, and consultation with the most eminent physicists of his acquaintance, he ventured to submit to the Royal Society, on the 20th February 1882, a Paper "On the Conservation of the Solar Energy." He had already, in a lecture on fuel, given before the British Association in 1873, made some profound remarks on the nature of combustion, and the utilization of different forms of energy, and had foreshadowed some ideas as to the sun; these had excited much attention, and he now carried them farther. He began by pointing out what an enormous amount of heat was continually being radiated away from the solar surface, and remarking that it had long been a source of wonder to natural philosophers how this could be given off, year after year, without any appreciable diminution of the sun's temperature. Various theories had been offered in explanation, but these were all more or less open to objection; and he put forward a new explanation which he conceived would be more satisfactory. This explanation was found in three postulates.

He assumed, in the first place, that gaseous matters, particularly aqueous vapour and carbon compounds, were present in a highly attenuated state, in stellar and interplanetary space; an assumption for which he brought forward several ingenious arguments. He next conceived that these gases were capable of being dissociated by the radiant solar energy. And thirdly, he attributed a peculiar effect on these dissociated vapours to the mechanical action of the sun's rotation. He likened this action to that of the well-known centrifugal blowing fan, which, revolving with great velocity, draws air in near its axis, and ejects it at its circumference. In this manner he argued that the effect of the solar rotation would be to draw in the dissociated vapours upon its polar surfaces, and so to subject them to intense combustion, after which they would be again ejected into space from the quickly revolving sun's equator. This "fan-like action," as he called it, when combined with the chemical phenomena, he considered to offer a solution of the problem of the maintenance of energy. He considered in fact that the sun might be regarded as a gigantic specimen of one of his own regenerative gas furnaces, with however the unusual condition that the same materials of combustion were used over and over again. He carefully avoided the charge of having broached a "perpetual motion" impossibility, for he admitted there must be some loss of energy, without which indeed his assumed process could not go on; but he opposed the idea of enormous waste, an idea that was as repulsive to him in the solar system as it had been in the manufacturing economy.

The new theory was of course warmly discussed, and the President of the Royal Society, alluding to it in his annual address, remarked: "Upon the questions therein raised the last word has been by no means said; and whether the theory be ultimately established, or whether, like a Phœnix, it shall hereafter give rise to some other outcome from its own ashes, it will ever be remembered as having set many active minds at work, and will always have a place in the history of solar physics."

Societies, Addresses, and Lectures.—It would be omitting a prominent element of Sir William Siemens's life not to allude to his connexion with the many scientific and technical societies and institutions to which he belonged. Although so much occupied in his business, with his mind so full of elevated thoughts, and his hands so busy with important affairs, he could always find time for attending to these. He took pleasure in belonging to almost any and every society that had aims connected with his work or his studies; he was proud of his membership with them, he willingly took office in them; he never grudged the attention required for their concerns; he was a constant attendant at their meetings, and he aided them in every way in his power.

His connexion with this Institution will be more particularly noticed hereafter. It may be added that he was elected a Fellow of the Royal Society in 1862; that he was President of the British Association at the Southampton meeting in 1882; that he was President of the Institution of Mechanical Engineers in 1872–3 and in 1873–4, of the Iron and Steel Institute in 1877, and of the Society of Telegraph Engineers in 1872, and again in 1878; and that he was Chairman of the Council of the Society of Arts at the time of his death. In addition to these he belonged to several other minor societies, and he was always ready, when called upon, to give aid by lectures or addresses to any Institution whose object he thought praiseworthy.

There was no lack of appreciation of his merits; for, in addition to the many medals and prizes awarded to him by scientific societies, he received at various times many honourable and flattering marks of the important services he had rendered to the public welfare, and in April, 1883, he received the honour of knighthood from the Queen.

The close of Sir William Siemens's life, in the prime of his faculties, and in the midst of his labours, was very sudden and unexpected. Some of his most intimate friends had noticed, for a little time, that he had a look of fatigue which could not be accounted for by any worldly care or anxiety; but there was no suspicion of any bodily disease sufficient to require medical advice, and there was no relaxation of his ordinary work.

On the afternoon of the 5th of November, 1883, he had been to a managers' meeting of the Royal Institution, and was walking home with his friend Sir Frederick Bramwell, when, at the corner of Hamilton Place, in Piccadilly, he slipped at the curb-stone, and fell heavily on his side; but though a good deal shaken, he noticed no serious hurt, and continued the walk to his house. He continued to attend business as usual, but in a few days he was attacked by a severe pain in the side; worse symptoms afterwards set in, and on the evening of the 19th he passed quietly away. A post-mortem examination showed that there had been a fatal disease of the heart of long-standing, its influence having been aggravated by a slight rupture due to the fall. He could not have lived long, and it was a matter of wonder that the disease had not affected his general health at an earlier time.

The news of his death made a great sensation, and the Dean and Chapter of Westminster granted a public funeral service in Westminster Abbey, which was held on the 26th of November, the body being afterwards buried in the cemetery at Kensal Green. The event called forth, as might be expected, widespread manifestations of sympathy with his bereaved relatives, and of respect for his memory. Obituary notices, giving accounts of his life and works, appeared in journals and periodicals of all kinds, not only of a scientific, but also of a popular character.[1]

The character of the man may be fairly inferred from the details given of his life and works. A few general remarks may be added here.

As a man of science his high position was admitted on all hands. Without any affectation of abstruse learning, he had a thorough knowledge, theoretically, of the natural sciences with which he occupied himself, and was thus able to deal with them in practice, not empirically or tentatively, but on a firm basis of sound logical reasoning, which guided him unerringly in their applications. He was mathematician enough to work out the many difficult problems required in his work, and in physics and chemistry he was an acknowledged master.

As an engineer, it is enough to say that if the province of the engineer is "the art of directing the great sources of power in nature for the use and convenience of man," there have been very few men in the profession who could show a higher claim to the title. He was probably one of the most accomplished mechanics that ever lived; and when any important engineering task of unusual nature or magnitude was presented to him, such, for example, as the design of the "Faraday," his thorough knowledge of the principles of construction and the properties of materials, combined with his ready inventive power, made its accomplishment easy to him. It is impossible to read his mechanical essays, or his multifarious remarks, on all sorts of subjects, at the meetings of this Institution, without admiration of his remarkable ability in the profession he had attached himself to.

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[1] These notices showed forcibly the high place he occupied in public estimation, and some of them were of great merit. Among these may be specially cited those in *The Times*, written by fellow-officers with him in the Society of Arts; in *Engineering*, written by his private secretary; and in *Nature*, contributed by his most intimate scientific friend, Sir William Thomson. In Germany, the notice in the *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, by Professor Wedding, aided by his brother Werner, was very full and interesting. There was also an excellent account of his metallurgical labours in the *Engineer;* and a work lately published, "The Creators of the Age of Steel," by W. T. Jeans (London, 1884), contains a memoir that deserves honourable mention. The writer of the present notice has to acknowledge information drawn from all these sources, as well as data communicated by private friends.

Of his character as an inventor much has already been said, but the inventions which have been described or alluded to in this memoir have necessarily only been those of the most salient kind, and of the greatest importance in their results; they form only a small portion of the immense inventive labour of his life. He was one of the most prolific and versatile originators of novel combinations that the mechanical world has ever known, as may be judged of by the fact that there are no less than 113 British patents to which his name is attached. But even this list is far from being a complete register of his inventions, seeing that there were great numbers of minor novelties continually springing from his fertile brain, which either were merged in the processes of manufacture, or were described to the world without protection, or indeed may have had only an ephemeral object, and have been forgotten altogether.

His business position, combining with the legitimate profession of a civil engineer the occupations of a manufacturer and contractor on a very large scale, was somewhat abnormal, but the way in which this came about has been already explained. He saw with great acuteness at an early period of his life that, in a line of practice comparatively new, and likely to have a very large extension, his devotion to manufacture would open a much wider and more lucrative field for his powers than mere designing, and in this view he was supported by his brothers. Again, at a later period, when engaged in the study and perfecting of his great heating and metallurgical inventions, he found it would be out of the question to attempt to get the necessary preliminary trials, on a large and expensive scale, carried out in the trade; indeed, apart from the risk which deterred the manufacturers, he experienced a determined opposition by the uneducated and prejudiced workmen which alone would have crushed a less energetic and persevering man. He accordingly embarked, for the sake of these metallurgical inventions, in the actual extensive manufactures they referred to. These undertakings were not free from the risks which so often attend large commercial speculations in a new field, and in some instances they caused much anxiety, and not a little pecuniary sacrifice, to himself and his friends. But his energy and perseverance carried him through all difficulties, and in his later years it may be said that everything he did was successful in all points of view. He had the discrimination to surround himself in his business transactions with able coadjutors and assistants, whose aid he was always glad to acknowledge.

He had considerable literary power, and, although a foreigner born, he acquired a knowledge of English, and an ability of using it, which were very remarkable. He wrote a large number of Papers of great value, which will, probably, be republished in a collected form.

It now only remains to add a few words on Sir William Siemens's connexion with this Institution. It is curious that he did not join it early; he was not unaware of the benefit to be derived from association with others of his profession; but he was diffident of his own qualifications, and he attached himself, in the first instance, to some other societies of easier admission. He was elected an Associate on the proposition of Mr. (afterwards Sir) Charles Fox, on the 4th of April, 1854, and was transferred to the full rank of Member on the 11th of December, 1860. He was at that time engaged in manufacture, and could only have been considered eligible on the ground of very distinguished merit in the power and practice of engineering design. He was elected a Member of Council in December 1871, and at the time of his death stood next in rank for nomination as one of the four Vice-Presidents.

In the year 1879 he made a munificent proposition, which he had already shadowed forth in his Address, a year or two previously, to the Iron and Steel Institute. He had noticed the growth of several other societies representing various special branches of civil engineering, and, indeed, in his position as the first President of one of them, he had encouraged their formation, and clearly justified their *raison d'être.* It occurred to him that it would be a good thing to combine them together, and he offered to contribute the sum of £10,000 to form the nucleus of a fund for providing a suitable building wherein they might be housed. Difficulties in the mode of carrying out the scheme prevented this offer being accepted, but it received the warmest acknowledgment, and the idea of the combination was considered so promising that it may very probably be taken up more effectively at a future time.

In March 1883, Dr. Siemens delivered one of a series of lectures given at the Institution on the "Practical Applications of Electricity," the subject he chose being "The Electrical Transmission and Storage of Power."

He was much attached to the Institution, and attended it whenever it was possible. He did not present many complete Papers to the Transactions, but he took great interest in all the proceedings, and very often joined in the discussions, as the published reports show. His great mechanical knowledge and critical acumen made him a high authority, and his remarks were notable for their clearness and pertinence, and were always well received.

He was indefatigable in the personal aid he gave to the business of the Council, and it is almost superfluous to say that he commanded the respect and esteem not only of his brother officers, but of all the members who had the good fortune to know him.

A short time before his death the Council had decided to award to him the Howard Quinquennial Prize for 1882, in consideration of his important discoveries and of the valuable improvements he had effected in the manufacture of iron and steel, and the acknowledgment of the award was one of the last acts of his life. The manner in which the prize should be given was determined by his widow, who expressed a wish to possess a bronze copy of the celebrated group of "The Mourners," by J. G. Lough, originally placed in the Great Exhibition of 1851, and now in the Crystal Palace. This was accordingly presented to Lady Siemens by the Council, as an appropriate symbol of her sad bereavement, and as a last affectionate tribute to the memory of their much-regretted colleague.

HENRY TOMLISON was born at Walton-le-Dale in the year 1844, and on leaving school in 1858 was placed for five years with his brother, Mr. Charles Tomlison, at the Wolverhampton Waterworks. In 1863, before he was twenty years of age, he was appointed manager, and in 1869 engineer and manager of the Cambridge University and Town Waterworks, which position he filled till his death on the 23rd of June, 1884.

During his connection of 21 years with the Cambridge Waterworks Company he rendered most valuable service in extending and perfecting the works. Although young when appointed to take office, the Directors foresaw that he would develop into a man of great scientific acquirements and practical skill. His ability as engineer and manager to the Cambridge and Trowbridge Water-Companies earned him the entire confidence of the Directors and others connected therewith, by whom he was held in the light of a personal friend. His engineering skill was not confined to Cambridge alone. He devised and carried out the waterworks at Trowbridge, Melksham, Frome, and Devizes; and his was also the scheme for the Warminster Waterworks, and reconstruction of the Ely Waterworks. He was also the engineer to the Frome Local Board sewerage-scheme.

Mr. Tomlison was an acute-minded, active, and straightforward man, indefatigable in the pursuit of any matter which he took in hand, and very well informed in all the details of waterworks-management. He was especially versed in the distribution of water under the constant-supply system, and in domestic fittings for waterworks purposes. In 1867 he was retained by the Metropolitan Board of Works to give evidence before the Select Committee on the Metropolis water supply, as to the successful operation of the constant-supply system in Cambridge. His physique was cast in a small and delicate mould, but the activity of his mind betokened no hint that the professional draughts upon it were too great. His death therefore at the early age of thirty-nine caused surprise as well as sorrow.

Mr. Tomlison was elected an associate on the 6th of February, 1872, and was transferred to membership on the 30th of November, 1880.

CROMWELL FLEETWOOD VARLEY was the second son of Cornelius Varley, an artist and man of science, who took an active part in the mechanical committees of the Society of Arts, and was one of the founders, and, indeed, the originator of the old Water Colour Society. He was born in Westminster on the 6th of April, 1828, and was educated at St. Saviour's Grammar School, Southwark.

In 1846 Sir William Fothergill Cooke introduced Mr. Varley into the service of the Electric Telegraph Company, and he rose step by step until he became electrician and engineer-in-chief to the Company. In 1848 he received a small recognition from the Board of Directors on submitting to them a vacuum lightning protector, for guarding the instruments and insulated wires from destruction by atmospheric lightning.

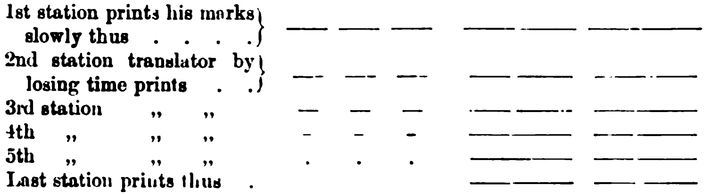
In 1854 he was called to the chief office, London, and subsequently became electrician; he had then provided the means whereby faults on a telegraph line could be determined, and the position located with remarkable accuracy. In 1854 he obtained his first patent for transmitting electric signals. This invention was known as the double-current system, and heralded marked progress in rapid telegraph signalling, and for the first time London was put into communication with Edinburgh direct, avoiding the necessity of re-transmission. The second invention was patented in December of the same year, and comprised several improvements in galvanic batteries for telegraphic purposes. It is interesting from the fact that among the claims is mentioned the use of induction plates for telegraphic and electric-light purposes.

In June 1855 the third patent for improvements in electric telegraphs was obtained. This invention comprised a system of working termed translating, in vogue at the present time with but little alteration from the original plans, and may be best described in the inventor's own words:—

"By translator I mean the apparatus used for bringing fresh batteries into play, to enable correspondence to be maintained direct between two stations, whose distance apart is too great to admit of connecting the wires into one continuous electric circuit. Generally speaking, it is not advantageous to attempt to work through more than 200 English miles of the ordinary telegraph wires suspended in the air in one electric circuit, but by arranging apparatus so that the current through the first electrical circuit shall bring fresh batteries into play, a second distance of 200 miles can be worked through, and so on; but there is a limit to this from the following causes:—1st, the electro-magnets used for putting the second set of batteries into play require a given time for magnetizing; 2ndly, the armature requires also a given amount of time to move toward the magnet to make the contact; 3rdly, an electro-magnet requires more time to magnetize than to demagnetize, thus in the case of a large electro-magnet, where the time required to magnetize it was 15 seconds, it only required 3 to demagnetize.

"The duration of a dot is not sufficient to magnetize the attracting magnets so fully as the long contact or dash, therefore the dot becomes shortened in a greater ratio than do the dashes, because the armature retires quicker in the former than the latter case. These actions waste time, and consequently the marks to be printed are shortened at each translating station. Therefore where many translators are in circuit, it becomes necessary to send slower, in order that the amount lost at each translation shall not absorb entirely the short marks (or dots).

A Circuit containing four Translators.


1st station prints his marks slowly thus . . . .
2nd station translator by losing time prints . .
3rd station ,, ,,
4th ,, ,, ,,
5th ,, ,, ,,
Last station prints thus .

"In this example the last station cannot read the sending of the first station, because the short mark (technically dots) have been so much reduced that they do not reach thereto. The dashes however, from their greater length, and from other causes, scarcely suffer."

After further describing the effect of induction in submarine cables, such as those between England and Holland, in which the effacement of the dot necessitated still slower signalling, he further points out how the land Morse-system can be made to translate into the submarine cable Morse-system without diminishing to so great an extent the rate of sending.

It would occupy too much space to fully describe the details whereby this was effected. This invention, however, made direct communication between London and Amsterdam for the first time practicable. It was subsequently still further extended, and direct working was obtained between London and Berlin, and London and St. Petersburg, which otherwise would have been practically impossible. He was contemporary with Sir W. Siemens in the invention of the polarised relay, and during his connection with the Electric and International Telegraph Company, he obtained numerous patents for improvements in electric telegraphs, and raised the general efficiency of the telegraph lines by re-insulation of the line with insulators still very extensively employed. In thus improving the electrical conditions of the line, long distance telegraphy was obtained without relays. He also aided the manufactures of telegraph wire, and longer lengths of wire without splits or flaws were obtained. He also introduced the process termed killing, whereby any defect in the manufacture of wire was detected at the mills, and enabled the manufacturer to guarantee its efficiency; the wire was likewise less subject to breakage during frost, and a greater dip could be allowed without the wires coming into contact.

It is more chiefly in connection with deep sea long submarine cables that he devoted the remainder of his career after resigning his connection with the Electric and International Telegraph Company,

The value and importance of Mr. Varley's work has been everywhere acknowledged by his fellow-workers. In a statement drawn up by Sir William Thomson, F.R.S., M. Inst. C.E., dated April 12, 1873, the following paragraphs occur:—

"In 1854 Mr. C. F. Varley, in a Paper read at the British Association Meeting in Liverpool, drew attention to the slowness of rate at which signals could be sent through long submarine conductors of small section, as compared with the rate obtainable through equally long electric circuits in the air. He described apparatus he had invented, by which the speed of transmission for longer air-lines than had been up to that time worked through, might be sufficiently augmented to meet the requirements of commerce, and he indicated the true method of increasing the speed of long cables by the proper choice of transverse dimension of conductor and dielectric.

"In 1855 he invented and put into operation at Amsterdam and London his translating relay apparatus and submarine telegraph key, by means of which the English submarine cables were successfully linked together with the German system of telegraphs, and he thus established direct communication between London, Hamburgh, Berlin, St. Petersburg, and all the principal continental towns.

"In the same year he invented the gravity-battery, which, with a little modification, is the one generally used for testing and working long submarine cables.

"In 1856 he further improved the translating relay-apparatus in extensive use in England, on the Continent, and elsewhere. He also described a method by which the distance or locality of defects in telegraph circuits could be deduced by observations and calculations at one end only of the circuit.

"In 1858 he invented a method, now well known to electricians under the title of the 'loop-test,' by which the position of faults, the resistance or magnitude of which is continually varying, can be ascertained with great accuracy. This method, for the cases in which it is applicable, is the most accurate known, and in all such cases is invariably used.

"In the same year he invented what are known as electric probes, by which the exact position of large faults in telegraphic cables can be ascertained to within 1 foot of distance from the outside of cable without even removing or interfering with the wire or other covering.

"In 1860 he still further improved his translating relay-apparatus, and he invented a machine for generating without friction electricity of high potential; he also invented a method of reducing the effects of terrestrial or auroral currents through long circuits, and protecting telegraphic cables from lightning. In the same year, in a Paper read before the Royal Society on the 'Retardation in Telegraphic Cables resulting from the Electrostatic capacity of Gutta Percha,' he described an apparatus he had invented in 1858, by which the speed of transmission through various cables could be accurately compared by this means. He had proved by experiment, in 1859, that the speed through long submarine cables varied inversely as the square of the length, a law which had been theoretically deduced by me in a Paper read before the Royal Society in 1855.

"In 1861 he invented a new form of insulator, discovered the causes of wholesale failures of those previously used, and by this means made it possible to work from any one part of Great Britain to another, no matter how bad the weather was. These insulators are now used on the lines of the English Government, who have more than two millions of them upon their poles. In the same year he constructed what is known as his artificial submarine telegraph cable, which is an exact electrical representative of a long submarine circuit. With this apparatus the electrician can study in his laboratory, as well as he could upon an actual cable, the various phenomena which present themselves, and can test any new inventions proposed for working such circuits. I have found it exceedingly useful and valuable for testing instruments for the Eastern and other great submarine lines.

"In 1862 he made a series of most important inventions by which the effect of earth- or auroral-currents (which are extremely troublesome in long submarine cables) are in most cases rendered entirely insensible. The chief of these inventions, the application of condensers for the transmission and reception of telegraphic signals, has the further advantage of largely increasing the distinctness of the signal, and the speed with which communications through long circuits can be maintained. It is now in general use upon all deep-sea cables.

"In 1865, in conjunction with myself, he constructed the apparatus known as the curb-key. The use of this key gives great distinctness to the signals, increases the rate of transmission, and reduces the errors.

In 1866 he invented a means by which negative electrification is very conveniently applied to preserve the conductors of defective cables from corrosion, and in consequence of this invention the lives of such cables can be very considerably prolonged. Its value was most conclusively proved last summer. When the fault which had existed in the French Atlantic cable for three years was cut out, the conductor was found to be perfectly intact, a result certainly due to the protection it had received through Mr. Varley's invention. This defective but protected cable was the only unbroken circuit between America and Great Britain, and carried all the messages for five months during the winter of 1871 and 1872. Without this protection of negative electrification the conductor is to some degree eaten away every time a positive current flows from it through the defect into the sea. When once the conductor is eaten asunder, further communication through the cable is impossible.

"In 1859 he was elected one of the members of the Commission appointed by the Privy Council and the Atlantic Telegraph Company to inquire into and report upon deep-sea submarine telegraphy. In the reports drawn up by that Commission, Mr. Varley published useful tables for calculating the speed of electric signalling through submarine telegraph cables of various dimensions.

"He has invented many other useful apparatus, has made original investigations in science, and has contributed very largely to the practical development of submarine and other telegraphs. After the failure of the first Atlantic cable in 1858, the late Mr. Robert Stephenson, who was then Chairman of the Electric and International Telegraph Company (of which Mr. Varley was electrician), became an adviser of the Atlantic Telegraph Company, and Mr. Varley undertook the duties of electrician without remuneration up to the year 1864.

"In the year 1863 he assisted Sir W. Fairbairn, Sir Joseph Whitworth, Captain D. Galton and myself, in the experimental determination of the proper form and material for the external covering of the Atlantic cable, and by means of his artificial cable assisted in estimating the dimensions proper for the conductor and insulator.

"In 1864–5 he tested, for the Atlantic Telegraph Company, the whole of their cable. He accompanied the expedition as their chief electrician, and has been connected with each subsequent cable which has united Great Britain and America.

"In 1865 and 1866 he and I were deputed by the Telegraph Construction and Maintenance Company to draw up new plans for testing the 1866 cable, which was successfully laid, and is now working. The apparatus we invented and adopted has been regularly used in the ship and shore testing during the laying of all long and many short cables laid since that date.

"He began his career in 1846 as electrician, when the Electric Telegraph Company was established, and rose up gradually by his own unaided exertions in their service. In 1852 he was appointed their Resident Engineer in London; in 1858 was appointed Chief Electrician, and in 1861 he became the Engineer-in-Chief. He remained in the service of that company until their telegraphs were purchased by the Government.

"He has laid and repaired various cables in different parts of the world, and has contributed several improvements in the machinery used in such operations.

"In 1861 he invented and constructed an automatic apparatus called the chronopher, by which the electric time signals which the Astronomer Royal had established between Greenwich and London are accurately re-transmitted over the whole of Great Britain. It is through this apparatus, which is purely automatic, that the clocks of nearly all the railway companies of the United Kingdom are corrected every morning, time-guns fired, and time-balls dropped in the various towns."

The generous testimony thus afforded by Sir William Thomson has been further supported by the late eminent Astronomer Royal, Sir George B. Airy, who, writing to Mr. Cyrus Field, so intimately connected with Atlantic telegraph enterprises, under date May 27, 1873, says:—

"I understood from Mr. C. F. Varley that he is desirous I should state to you the nature of the connection which I had with him some time ago, in regard to the use of galvanism for the purpose of the Observatory, and especially for the dissemination of signals of accurate time. I can hardly say when I was first in communication with Mr. Varley; whether before I had established galvanic connection with London Bridge, or after that time. I remember well that in all which took place after that time for the accurate distribution of the signals originating from this Observatory, Mr. Varley effected (gratuitously) almost everything. The spreading abroad of these signals was a favourite object with me, and quite as much so with Mr. Varley. We consulted him on almost every point, and finally, by his invention of the Chronopher (an apparatus which, as I understand, the increasing popularity of the signals has made it necessary to extend), he gave a degree of accuracy to the operations which never could be given before by anything but a self-acting machine. The establishment of the time-signal system, as an accurate and valuable institution, is almost entirely due to Mr. Varley.

"In what I have stated above I refer to matters which passed under my own eye and within my personal knowledge. But for all that I have read in the writings of Du Moncel and others, I fully believe that a much higher praise is due to Mr. Varley for the improvements which, guided by a theory of great generality, he has introduced into the operations of telegraphy in general, and sub-oceanic telegraphy in particular."

The important service which Mr. Varley had rendered in making deep-sea telegraphy practicable, and in ensuring the confidence of the subscribing public to take a pecuniary interest in an enterprise held by many at the time to be foolhardy and chimerical, would doubtless have received some suitable recognition for his services, which culminated in uniting America with England, but for an unfortunate accident explained in the following extract from the *Times*, August 29, 1866:—

"As soon as the cable was laid, the North Sea expedition to connect England with Prussia took Mr. Varley out of the country. On his return he found, in the public recognition of those who had contributed to the success, not a word about his long, arduous, and successful services. *Sic vos non vobis.*

"The public work has been accomplished, and probably the public are not very particular, so long as they are served, who meets the reward. Yet after all, it is of some consequence that men who devote conspicuous ability and scientific ardour to an achievement of world-wide importance, should not be made to feel that rewards are less the consequence of merit than a happy accident."

From this time forward he devoted his chief energies to improving the speed by which signals could be transmitted through long submarine cables, and united with Sir William Thomson and Professor Fleeming Jenkin in working their several patents for this purpose; and by the employment of curved signals, condensers, and the Thomson recorder and mirror galvanometers, the present successful working of long submarine cables was and is rendered successful. The condenser completely cuts off the effect of earth-currents, and adds materially to the distinctness of the signals.

He attended—and, when his health permitted, took an active part in—the proceedings of the Royal Society. He lectured on Long Submarine Telegraphing at the Royal Institution, and was a frequent attendant at the meetings of the Institution of Civil Engineers; he likewise was, as far as his failing health permitted, an active member of the Society of Telegraph Engineers.

Those who desire more fully to know the problems involved in long submarine telegraphy and their solution, will find full details in the work of the late Count T. du Moncel, entitled "Exposé des Applications de l'Electricité," Paris, 1872–78, in which the labours of Mr. Cromwell Varley are clearly set forth.

In 1870 he obtained a patent for a new mode of telegraphing by means of waves transmitted electrically through a conductor—an invention which anticipated the labours of Elisha Gray in harmonic telegraphy, and the subsequent work of Professor Graham Bell with his modification of Gray's receiver and the electric telephone, to which he gave the name of Cymaphen (*i.e.*, wave-writer); and to an apparatus employed in separating the systems of signalling he gave the term Echo-cyme (or wave-stopper).

From 1870 till the time of his decease he obtained several patents. Among some of his most recent work was that of electric lighting and improvement in the cymaphen. These latter he worked in conjunction with his brother, Frederick Henry Varley, and joint patents were applied for in 1881, for electric lamps and appliances connected therewith, also for improvements in cymaphens.

Mr. Varley was elected a Member of the Institution on the 5th of December, 1865. He died on the 2nd of September, 1883.

JOHN WHITFIELD was born at West Allendale, in the county of Northumberland, in 1827, being one of four sons of Mr. Joseph Whitfield. By showing an aptitude for drawing and mathematics at school, he was taken in hand by the late Mr. Thomas Sopwith, M. Inst. C.E., under whose kindly auspices he was introduced to his future profession, and for whom he ever expressed the greatest indebtedness and respect. He was advised to go to London, and for some time was in the office of Messrs. Smith & Scott, surveyors. Returning to Northumberland while still a young man, he carried out extensive and elaborate surveys of mining property, under the direction of Mr. T. J. Bewick, M. Inst. C.E. In 1856 he was appointed on the staff of the late Mr. W. M. Peniston, M. Inst. C.E., Chief Resident Engineer of the Pacific and São Francisco Railway in Brazil, and went out to Pernambuco in October of that year. He remained in Brazil until 1863, and during the latter part of the period was Mr. Peniston's chief assistant, and during the latter's absence officiated as his *locum tenens*. He returned to England in 1864, and assisted Mr. Bewick in getting up the Parliamentary plans of the Hexham and Allendale Railway. Subsequently he remained at home for about four years, engaged in various surveys and inquiries.

In January 1868 Mr. Whitfield received an appointment as third grade executive engineer in the Indian Public Works Department, and proceeded to Calcutta in the following month. He was at once posted to Raneegunge to assist in the completion of the plans of the Damuda canal, then under preparation by the late Captain Garnault, R.E. The original design of the canal was for navigation and irrigation, with a view to afford the collieries at Raneegunge water-carriage to Calcutta, 110 miles distant. The whole of the work was done in the hot season, from April to August, and Mr. Whitfield's services were gratefully acknowledged in the report made to Government. In fact without his valuable assistance and untiring zeal the report could not have been presented in one season. Owing to financial reasons, and partly to a practical joke played by a European convict, who was supposed to have discovered coal at Midnapore, the original plans were not adhered to. Some years later, however, the work was put in hand as an irrigation-canal, and Mr. Whitfield had the satisfaction of carrying out his own designs to a most successful issue. The benefits of the work, both financially and on sanitary grounds, have been admitted by the zemindars, and repeatedly acknowledged by the Government of Bengal. From 1869–79 Mr. Whitfield was in charge of the most important irrigation division in Calcutta, and was largely engaged in municipal matters concerned with the gas- and water-supplies, and the drainage of the densely populated suburbs.

With the exception of three months' privilege leave in 1876, Mr. Whitfield was always at work, and from repeated attacks of fever he suffered considerably. He was glad to have an opportunity of retiring when a general reduction took place in 1879, at which time he was an executive engineer, first grade. It was his intention to return to England, but at the last moment, as he was leaving Calcutta, an offer came from the trustees of the Durbungah estate to take up the duties of Chief Engineer. This was accepted, and necessitated residence in Monghyr. But in less than twelve months he was compelled by medical advice to return home for rest and change. The appointment was kept open in the hope that renewed health and strength would enable him to return. The relaxation had, however, come too late. He took up his residence at Weston-super-Mare for the benefit of sea air, and after about three years, and enduring several months of weakness and pain, he quietly passed away on the 2nd of January, 1884.

Mr. Whitfield was too reserved to be popular, and much too hard working to be a general favourite among his subordinates. He was a true friend, and sincerely respected by many natives of India, who saw enough of his life to appreciate his good qualities. He was elected a Member of the Institution on the 2nd of February, 1864.

EDWARD MATTHEW PARK, third son of the late eminent sculptor Patric Park, R.S.A., was born in Edinburgh on the 16th of November, 1850. In early boyhood a pupil of John Watson's Institution, Edinburgh, from which he was removed in 1861 to a London school, and in 1865 to one in Lausanne, Switzerland, Mr. Park served an apprenticeship in a lawyer's office in Inverness, which he left in July 1869 for New Zealand, intending to become a sheep-farmer. Arrived in the colony, he went to live at Ashburton, in the province of Canterbury, with his uncle Robert Park, late Surveyor General to the New Zealand Government. In the year 1871 the New Zealand Government, under the premiership of Mr. (now Sir) Julius Vogel, were beginning the construction of the railway system which has contributed so largely to the prosperity of the colony, and Mr. Park entered into an apprenticeship to Mr. John Carruthers, M. Inst. C.E., his uncle on the mother's side, who then held the position of Chief Engineer to the Government. On the completion of his apprenticeship he was appointed Assistant and afterwards Resident Engineer in the Public Works Department, and was engaged on various works, including the tunnel through the Rimutaka range, and the Rimutaka incline. Returning to this country, he was sent out in 1881 by Messrs. Wilson Sons & Co. to the Great Western Railway of Brazil, where he was employed on the construction of a branch line of that system which leaves Parahyba for the interior of the country. On his return from Brazil, Mr. Park went out to Venezuela in 1883 as Chief Assistant under Mr. Lidiard, to take the trial-survey for a line from Las Tablas, on the mouth of the

Orinoco, to the Guacipati mines. This trial-survey was completed by him towards the latter end of October of the same year, during the absence of Mr. Lidiard in England. While engaged upon this work, Mr. Park contracted those seeds of malarial poisoning which ultimately proved fatal to him. He died at Inverness on the 7th of April, 1884, at the early age of thirty-three, having borne his last illness with the same unvarying cheerfulness and unselfishness which during his short but useful and happy life had endeared him to a large circle of friends.

Mr. Park was elected an Associate Member of the Institution on the 4th of February, 1879.

SECT. III.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

Modified form of Ertel's Dioptric Eye-piece.

By C. A. VOGLER.

(Zeitschrift für Vermessungswesen, vol. xii., 1883, p. 477.)

Next to the form of Huyghenian eye-piece, recommended by Dr. Jordan,[1] this pattern ought to be noticed. Reichenbach, in order to give his telemeter as small a constant as possible, fitted the telescope with two simple Keppler eye-pieces. Ramsden's eye-piece had not sufficient field of view, while the Huyghenian was believed by astronomers unsuitable for the micrometer eye-piece, and is only achromatic by the two lenses being placed at a fixed distance apart. Ertel, though aware of this, supplied the latter to Reichenbach's telemeter, and yet succeeded in retaining its previously small constant. Its achromatism was preserved while the diaphragm was made movable.

Dioptric eye-pieces were made by Ertel's successors which admitted of two movements, the focussing of the eye-piece as well as the movement of the diaphragm, and though one of these was useless, they proved that at short terrestrial distances achromatism was unimportant. On the other hand, carefully repeated experiments in Switzerland and Bavaria proved that telemeter-constants varied largely by reason of an unintentional motion in the position of the diaphragm. It was therefore highly desirable to remove altogether the cause of this motion by having the ring on which the diaphragm-disk rests turned out of the same piece of metal as the eye-piece tube. But in order to give the constant a prescribed value, the lens attachment was fitted with a longish screw working in a toothed box, and so, after the eye-piece was focussed, a small adjustable motion was given to the lens towards the cross-hairs. The relative positions of each was thus made safe.

The working drawing for an eye-piece of this kind, made in 1871, is given, and the Author is gratified to find that similar eye-pieces have been for a long time fitted by Sprenger (of Berlin) to levelling instruments for railway work, and that no complaints as to their imperfect achromatism have been made.

E. H. C.

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[1] Dr. Jordan adds in a footnote that Randhagen of Hanover has also communicated to him a construction very similar in principle to the above, for a second telemeter he has in hand.—E. H. C.

Semicircular Protractor and Sliding Set-Square.

By T. Welizki.

(Zeitschrift für Vermessungswesen, vol. xii., 1883, p. 252.)

This method of plotting is preferred by almost all Russian surveyors. Supposing it be required to plot a line through a given point at a certain angle with the meridian of a plan, a semicircular protractor, fitted with a vernier, is used in conjunction with a 60° set-square and a ruler; the alidade is set by the vernier at the required angle, and at the same time the alidade line is laid on the meridian of the plan. The set-square is then laid against the protractor by the ruler, and slided along the latter until its edge passes through the given point, when the line drawn through the point will form the required angle with the meridian.

To find the bearing of any line upon the plan, the operation must be done in the reverse way.

E. H. C.

On the Strength and other Properties of Cuban Woods.

By Esteban Duque Estrada.

(Van Nostrand's Engineering Magazine, Nov. 1883, p. 417.)

The timber trees of Cuba are remarkable for their great height and girth, their almost perfect indestructibility by insects, dry-rot and decay, their hardness and homogeneousness, beauty of texture and grain, and their fragrance.

The following are the properties of a few of the principal kinds:—

Babia (*Cordia geräscanthoides*), attains a height of about 60 feet, and diameter of 18 inches. The colour is dark greenish brown at the heart, lighter in the sapwood. The wood is strong, durable, and light, and is used for framing, carriage-making, and general house-fitting. Sp. grav., 0·78.

Dagame (*Colycophyllum candidissimum*), is very plentiful. Height 40 to 50 feet. The largest section that can be obtained after squaring, is 12 inches. Sp. grav., 0·90. The wood is of a pale yellow colour, very fibrous, close-grained, resembling boxwood; is very strong and elastic, easily worked, turns remarkably well, is entirely free from knots, susceptible of good polish, and is very durable. It is used extensively in general house framing, and joiners prefer it for their work to most other wood. It is also used for carriage-building and for ships' yardarms.

Guayacancillo (*Guaiacum verticale*), resembles *lignum vitæ*, and is used for similar purposes. Sizes from 10 to 20 feet long, and 6 to 10 inches square. Sp. grav., 1·08.

Jiqui Comun (*Bumelia nigra*), attains a height of 50 to 70 feet, and scantling of 18 inches. Sp. grav., 1·20. It is one of the hardest woods known, is fine-grained, and very compact. It has a rosewood colour, becomes harder and darker with age, when very old being almost black, and is very difficult to work. It is a very good wood for posts, dock-piles, telegraph-poles, scaffoldings, railways, and main posts of gateways. It turns easily.

Jucaro Prieto (*Bucidæ*), can be obtained in logs 36 feet long, and 16 inches square. Sp. grav., 1·08. The wood is a dark brown colour, resembling black walnut, is very strong, tough, and elastic, fine grained and free from knots. It stands the weather well, is easily worked, and takes a good polish. It is largely employed in naval constructions where strength and durability are required. It is also extensively used by millwrights, and is an excellent material for posts, piles, and general dock-construction.

Quiebra-hacha (*Copaifera humenæfolia*), can be obtained in logs 50 feet long and 24 inches square. Sp. grav., 1·30. Its colour is dark red, like the darker kinds of mahogany. It is very hard, yet not very difficult to work. It has no equal for use underground and under water. It is used for all kinds of posts, sleepers, piles, dock-beams, engine foundations, and framing of heavy machinery.

Sabicu (*Mimosa odorantissima, vel Acacia formosa*), yields timber from 30 to 40 feet long and up to 36 inches square. Sp. grav., 0·95. It has very little sapwood, and its bark is an excellent material for tanning. It is hard, heavy, strong and durable. The wood is used for shipbuilding in Cuba, and also in England and in Spain, for beams, keelsons, engine-bearers, sternposts, cleats, &c.

Some other woods are described in the Paper, a Table of tests for determining the modulus of elasticity of the various kinds is given, and also a description of the testing-machines employed.

W. H. T.

A Uniform System for Tests of Cement.

(Transactions of the American Society of Civil Engineers, March 1884, p. 53.)

A preliminary report of the Committee of the American Society of Civil Engineers, on a uniform system of tests for cement, has been published, formulating the mode of testing recommended for adoption. The form of briquette recommended is substantially that adopted by Mr. John Grant, M. Inst. C.E., which he had found to be the form that gives the most uniformly consistent results. The briquette is moulded to a uniform thickness of 1 inch, and a length of 3 inches; in profile, it is 1¾ inch wide at each end, and is indented at the middle of its length to a width of 1 inch, with a ¾-inch radius of curvature. The briquette is therefore 1 inch square at the middle and narrowest part. The shoulders formed by the indentations afford bearing surface for the testing clips. It is recommended that the tests should only be for tensile resistance. The recommendations for testing are, summarily, as follows:—

Samples of cement for testing to be taken from the interior of original packages, and stored in tightly-closed receptacles until required. Each sample should be thoroughly mixed. The percentages of weight of each sample rejected by sieves of 2,500, 5,476, and 10,000 meshes to the square inch, are ascertained, the wire-gauges of the sieves being respectively No. 35, No. 37, and No. 40.

Sand for mortar that does not pass a sieve of 400 meshes, and sand that passes a sieve of 900 meshes, to be rejected. The proportion of sand is to be weighed out, and intimately mixed dry with the cement; all the water to be added should be added at once; the mass to be moulded before setting begins. Water in an atmosphere of between 60° and 70° Fahrenheit should be used.

The bulk of given weights of cements is ascertained by filling 5 ounces, if of Portland cement, and 4 ounces, if American hydraulic cement, into an upright cylinder 6 inches high, and having a sectional area of two square inches; and by lowering on to the cement a closely fitting frictionless piston, weighing with its attachments 50 pounds. For sand, ascertain the weight of a volume equal to that of the cement with which it is to be used, the sand to be shaken before being weighed, until its volume remains unchanged.

For Portland cement, to make five briquettes, composed of 1 part of cement and 3 parts of sand by weight, with 10 per cent. of the combined weight, of water; ten briquettes of neat cement, with from 20 to 25 per cent. of water, and two cakes of neat cement 2 or 3 inches in diameter, $\frac{1}{2}$ inch thick, with thin edges. To note the time taken by the cakes, mixed with water to a stiff consistency, to set hard enough to resist the Gillmore wire-test:—a $\frac{1}{12}$-inch wire loaded with $\frac{1}{4}$ pound, and a $\frac{1}{24}$-inch wire loaded with 1 pound. One of the cakes should be immersed in water, and examined from day to day, for constancy or variation of volume. The moulds to be removed when the briquettes are hard enough to admit of it; and, for the sake of uniformity, the briquettes should be immersed in water at the end of 24 hours. They should be tested immediately after being taken out of the water, in a temperature of between 60° and 70° Fahrenheit.

In testing, to commence with a stress not exceeding half the probable strength of the test-piece, and to continue the test by adding regularly to the stress, at the rate of 200 pounds per minute, until rupture takes place. The five briquettes of cement and sand should be broken at the end of 28 days, five of the briquettes of neat cement at the end of 7 days, and the remaining five after 28 days. The average results of each group of five to be taken.

For testing American hydraulic cements, briquettes are to be moulded as for Portland cement, except that for cement and sand mixtures the proportions are equal parts by weight. Half of the briquettes of neat cement should be broken at the end of 24 hours, the other half in 28 days, and the mixed briquettes in 28 days.

In the course of discussion, it was explained by Mr. D. J. Whittemore, the President, and a member of the Committee, that to provide against the influence of humidity on the materials, samples as already noted were expressly taken from the interior of the packages, and kept in close boxes until they were required. Mr. Owen found in his experience that the sectional area of briquettes was in many cases reduced by air-bubbles, which could only be eliminated by industriously kneading the cement. Mr. Worthen, having had to use a cement not quick-setting, mixed with it plaster of Paris, which made a very hard cement. Mr. Whittemore stated that whilst there were, as he estimated, three and a quarter million barrels of American hydraulic cement used annually, there were only used half a million barrels of Portland cement annually.

D. K. C.

Cork as a Building Material. By W. Wagner.

(Centralblatt der Bauverwaltung, 1884, p. 239.)

In this Paper the Author points out the advantages to be derived from the use of cork for building purposes, and describes a brick produced by a firm in Ludwigshafen, of which this material forms one of the chief ingredients. The shape in which the cork is utilized is that of an aggregate in a kind of concrete which is afterwards formed into bricks. The cork employed consists of chips reduced by machinery to the size of peas. The matrix used for producing coherence between the particles of the aggregate is composed of lime and clay. After the concrete has been prepared it is pressed into the required sized bricks, which are baked in kilns at a temperature of 120° to 150° Centigrade (248° to 302° Fahrenheit), the result being an extremely porous stone of very low specific gravity. Bricks produced in this manner, and of the same form and dimensions as the German normal brick (9·8 inches × 4·7 inches × 2·5 inches), weigh only 600 grams (1·3 lb.), with a specific gravity of 0·3 (specific gravity of cork 0·24). The mean compressive strain they are capable of withstanding is 2·8 kilograms per square centimetre (40 lbs. per square inch). A German normal brick weighs from 8·4 to 8·8 lbs., and possesses a resistance against crushing varying from 4 to 7 kilograms per square centimetre (57 to 100 lbs. per square inch). Cork stone is a very indifferent conductor of heat, and is capable of withstanding a temperature of 180° Centigrade (356° Fahrenheit). The chief advantages of the material are its small weight, capacity for being moulded into bricks of every form and size, that the bricks can be readily cut or sawn, and will take nails and screws, and form a good surface for plastering, as well as resist great heat. On account of these qualities it is remarkably well adapted for damp-proof courses, lining buildings to neutralise effects of temperature, forming unsupported walls in upper stories, &c.

J. R. B.

Mechanical Method of Solving Engineering Formulas.[1]

By Lala Ganga Ram, Assoc. M. Inst. C.E., Exec. Eng., P.W.D., India.

To effect this, three instruments have been made adapted to solve the equations used in designing timber beams and trusses, retaining-walls and iron girders. For each of these purposes there is a boxwood rule[2] with sliding scales graduated to show the several factors of the equations used, and by the sliding and setting of one scale against the other, the required result is obtained.

No. I. For finding the scantlings of beams, joists and roof trusses. Solves the equations for transverse strength and deflection, and for strains on rafters and ties of roof trusses of various shapes.

No. II. For finding the thickness of retaining-walls, level topped and surcharged. In this case the instrument gives the thickness of a rectangular wall in statical equilibrium under the action of various pressures. This result is then modified, according to directions on scales, to suit different forms in practice, such as trapezoidal, vertical faced, vertical backed, buttressed, &c.

No. III. For finding the strains on plate and braced girders. This instrument is contrived to find the following strains:—Bending strains at any point of the girder, that at the centre corresponding to the formula $\frac{W\,L}{8\,D}$. Shearing-strains on any bar or at any point, due separately to uniform and moving loads, and thus showing where counter-bracing is required.

Fuller description and illustration of the scales, their method of using and the principle on which they are based, are given in the Author's pamphlet.

G. R.

Testing-Machines. By Messrs. Denizeau and Lechien.

(Memorial de l'Artillerie de la Marine, vol. xi., 1883, p. 311.)

The first part of this long and copiously-illustrated paper deals with the principal methods and machines employed for the purpose of testing the strength of materials. At a future date, the second part is to be published, treating of the results of trials and circumstances influencing them, of a classification of iron and steel from their mechanical properties; of matters specially relating to the use of metals for ordnance.

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[1] The original is in the Library of the Inst. C.E.

[2] Made by Stanley, Great Turnstile, Holborn, London; a complete set has been presented to the Inst. C.E. by the Author.

The following order is adopted in the description of testing-machines:—

I. The methods adopted to produce and measure the effort.
II. The arrangements and devices for holding the specimens.
III. Apparatus for measuring the change of form of the test-piece.

Testing-machines may be divided into four classes. Machines in which the effort is exerted:

(1) By weights acting directly and so measured.
(2) By weights acting through levers and so measured.
(3) By a hydraulic press and measured by a steel-yard.
(4) By a hydraulic press and measured by a manometer.

(1) Hodgkinson, Vicat, Morin, and Tresca, all made numerous experiments upon the strength of materials by directly applying the loads. For workshop practice there are obvious objections to this mode of procedure. The inconvenience of dealing with heavy weights, the difficulty of applying them without shock to the specimen, and of exactly reading the elongation of the test-piece, need only be mentioned to explain why this system is now scarcely, if ever, adopted.

(2) Simple-lever machines, of which four examples are given, have many advantages, but it is impossible to be absolutely certain as to the exactness of the results. Even in the best machines the effort is exerted through a system containing variable factors—such as the friction of the knife-edges, their deformation in consequence of shocks at the moment of rupture of the specimen, change of position, and even change of form, of the levers themselves, &c. Three causes of errors are examined:—

i. The influence of sudden addition of weights. It is shown that if

E = the modulus of elasticity of test-piece.
ω = area of its cross-section.
λ = its statical elongation.
l = its length.

Then for an elongation = x—

$$\frac{P}{g} \times \frac{d x^2}{d t^2} = P - E \omega \frac{x}{l},$$

and since

$$P = E \omega \frac{\lambda}{l};$$

therefore

$$\frac{d^2 x}{d t^2} = g \frac{E \omega}{l} (\lambda - x),$$

and

$$\frac{d x}{d t} = g \frac{E \omega}{l} \left(\lambda x - \frac{x^2}{2}\right).$$

Thus a series of oscillations is produced the amplitude of which is 2λ. These oscillations have an effect on the test-piece which it is most important to avoid, and therefore on the Creusot machine the weight of the lever is taken off while an addition is made to the load. In another machine, by Mr. Mangin, this is effected by a screw.

ii. The non-verticality of the test-piece has an effect which in the case of a single lever instead of the real load on the test-piece being

$$T = P \frac{b}{a},$$

where P = weight applied, and b and a the lever arms, is

$$T' = P \frac{b \cos \alpha}{a \cos (\alpha - i)}.$$

Where α = angle of displacement of lever.
i = ,, ,, test-piece.

If c = the length of test-piece and clamps, then approximately—

$$i = \frac{a}{c + l} \times \frac{\alpha^2}{4}.$$

So that by making c as large as possible in proportion to a, the error is correspondingly reduced. In the Wadde machine an arrangement of two levers is employed. The second for any displacement (α) of the first makes an angle β with the vertical. The practical effect is to multiply the moment of the first by $\cos (\alpha + \beta)$, and that of the second by $\cos \beta$. The errors from this cause are thus almost entirely eliminated.

iii. The displacement of the centre of gravity of the levers with the usual mode of fixing the knife-edges causes the equation in the normal position.

$$T a = P b + p d \cos i,$$

where i = angle of centre line of lever with line joining knife edge to centre of gravity.
p = weight of the lever.
d = distance at the centre of gravity from knife edge to become—

$$T' a \cos \alpha = P b \cos \alpha + p d \cos (i + \alpha)$$

after turning through an angle = α.

This error is obviated by either making $d = o$, or $i = o$, or, as in the Creusot and other machines, making $\alpha = o$.[1]

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[1] This is also arranged for in a most ingenious manner in the Fairbanks (American) testing-machine. Van Nostrand's Engineering Magazine, April, 1884.

(3) Of machines in the third class six are described and illustrated, amongst them those of Tangye, Kirkaldy, Flad, and Pfeiffer. The importance of avoiding any hammering action of the pump is insisted upon, and the advantage of the Desgoffe machine, which effects this by a screw compressor, is pointed out. Also the great care which should be taken to avoid any shocks upon the knife-edges when the test-piece breaks, and the necessity for frequently verifying the reading of the scale-beam, as well as the difficulty of doing this at all in machines of some constructions, are points duly discussed.

(4) Amongst machines in which a manometer or pressure-gauge is employed to read the loads are those of Whitworth, Desgoffe, Thomasset, Chauvin and Marin-Darbel, and Maillard. The last-mentioned is considered to be extremely reliable, and is very extensively employed in tests for the metal used for ordnance.

A description of the various modes of holding test-pieces of different forms is given, followed by an account of the methods of measuring the alteration in the form of the test-piece.

A discussion of the machines and apparatus for compressive tests on bars and tubes, and for testing the effect of bending, torsion, and of live loads upon rails, springs, trunnions of guns, &c., conclude the first part of the article.

H. S. H. S.

On the Constants for Strength in Tension and their comparative Arrangement for different Materials. By E. Hartig.

(Der Civilingenieur, vol. xxx., 1884, p. 93.)

The Author considers that the question as to the best standard of comparison for judging of the quality of constructive materials still awaits settlement, and that the system proposed by Wöhler, and since 1879 adopted by the German Railway Union, of judging the quality of iron and steel by the sum of the tearing-strength per unit of original sectional area, and the contraction per cent. of the sectional area of fracture is irrational.

There can be no doubt that for purposes of comparison the work required for tearing, applied to the unit of volume or weight, is decidedly preferable, as it has a distinct scientific meaning.

The contraction at the place of fracture is a local phenomenon occurring after the maximum stress has been attained, and is dependent on the length of the test-piece; the latter should be much greater than is generally the case, long enough to neutralise the effect of the method of securing the ends of the test-piece on the total extension.

That the contraction of the area of fracture is a phenomenon occurring after the maximum stress has been attained cannot, with the testing-machines ordinarily in use, be demonstrated; but this may be done by an apparatus in which an approximately perfectly elastic body is used to measure the load, in combination with an arrangement for automatically registering the results in the form of a diagram, as in the testing machine of Reusch.[1] Diagrams from this machine, in which the abscissas represent the extension, and the ordinates the load on the test-piece, show clearly that in most cases stress increases with extension up to a certain point, and beyond this, while extension increases with greater rapidity until severance takes place, the stress decreases. Previous to the attainment of maximum stress, the whole of the material appears to participate equally in the extension, while afterwards the latter is confined to a short length, within which the area is contracted and fracture occurs.

According to the preceding, breaking stress and breaking extension should be defined as follows:—

Breaking-stress is the greatest stress occurring during the test, and breaking-extension the total extension (partly elastic, partly permanent) at the moment of maximum stress.

According to the Author's proposal, the work required for tearing a test-piece of 1 gram in weight would be expressed as follows, by means of quantities obtained with the help of testing machines of the kind above referred to:—

$$A = \eta \frac{\delta}{100} R \text{ metre-kilograms,}$$

where $R = N . P$;

$N = \frac{\text{Length of test-piece}}{\text{Weight of test-piece}}$;

P the breaking load;

η a coefficient obtained from the diagram giving the ratio of the area included in the diagram to that of the circumscribed rectangular parallelogram;

δ the relative extension in percentage of the original length.

This system avoids the necessity of measuring the sectional area; where, however, there is no difficulty in doing this, the work A may be expressed thus—

$$A = \eta \frac{\delta}{100} \frac{a}{s},$$

where s is the specific gravity of the material, and a the stress per square inch of the original sectional area.

The Author proposes to measure the stress, at the limit of elasticity, for instance, and the modulus of elasticity, by the length required of a piece of the same sectional dimensions as the test-piece, to produce by its own weight the same stress and an extension equal to the original length respectively (assuming the condition of perfect elasticity to extend so far).

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[1] *Vide* "Civilingenieur," 1879, p. 585.

The Author considers that for the purpose of classifying iron according to the work required for the destruction of a test-piece of a given weight the material should first be reduced to a perfectly definite condition by thorough annealing. He has found that different pieces of the same kind of iron wire, treated in this way, showed a remarkable agreement in the values obtained for δ, R and A.

By this system of testing it would be possible to use comparatively small and cheap testing machines, as the test-pieces would take the form of wire, and the frequency of flaws in the material would be shown by the number of breakages in the process of drawing.

The Paper is illustrated by diagrams and tabular statements of the results of experiments.

G. R. B.

On the Strength of Wrought-iron Struts. By J. Christie.

(Transactions of the American Society of Civil Engineers, April 1884, p. 85.)

An extensive course of experiments were made at the Pencoyd Iron Works, Pencoyd, Pa., to determine the comparative resistance to compression of struts of rolled angle, tee, beam, channel, and tubular sections of iron. The angles and tees ranged from 1 inch by 1 inch to 4 inches by 4 inches; the channels from 2 inches to 12 inches; the beams from 4 inches to 6 inches; and the tubes from $2\frac{1}{3}$ inches to $3\frac{1}{2}$ inches in diameter. The lengths of specimens varied, from 6 inches to 15 feet.

The pieces were tested in a Fairbank testing-machine by four methods—with loose flat ends, with fixed ends, with hinged ends, and round ended, of which the ends were fitted to balls resting on flat plates. The hinge-ended and round-ended specimens were arranged so that the centre of pressure was coincident with the centre of gravity of the section. Slight deviations from the coincidence here designed, reduced very much the resistance of the pieces. When pieces were straight and accurately centered, the hinge-ended struts were fully as resistant as the flat-ended struts. In all instances, the bars bent in the direction of the least radius of gyration of the section, except in the case of very short pieces, which failed by direct crushing. When the lengths of flat-ended bars were less than 231 times the least radius of gyration, all the specimens were permanently bent or set after failure; but, for lengths above 370 times the radius, all the specimens recovered their original condition. With fixed ends, the corresponding length-ratios were 174 and 312; with hinged ends, 162 and 254. When the ratio of the length to the least radius of gyration was reduced to 30, the bars ceased to bend in a regular curve, and failed by irregular crippling. When the struts were short—say, less in length than 20 times the least radius of gyration—there was no practical difference in the strength of the four classes of struts.

The following selection from a Table of average results of the tests, shows the general bearing of the proportions of struts on their resisting power :—

AVERAGE RESULTS of TESTS of STRUTS.

Ratio of Length to least Radius of Gyration.	Ultimate Compressive Resistance in lbs. per square inch.			
	Flat Ends.	Fixed Ends.	Hinged Ends.	Round Ends.
Ratio.	lbs.	lbs.	lbs.	lbs.
20	46,000	46,000	46,000	44,000
40	40,000	40,000	40,000	36,500
80	32,000	32,000	31,500	25,000
100	29,800	30,000	28,000	20,500
200	14,500	17,500	10,800	6,000
300	7,200	9,000	5,000	2,800
400	3,000	5,200	2,500	1,500
480	1,900	..	1,800	..

The following selection of instances of tests may serve to fix ideas and exemplify variations :—

Description.	Section.	Length.	Sectional Area.	Ultimate Stress.	Ratio of Length to least Radius of Gyration.
	Ins. Ins. Ins.	Feet.	Sq. Inches.	lbs.	Ratio.
Flat-ended L-iron . . .	4 × 4 × ⅜	15 0	2·96	12,400	228
" " . . .	3 × 3 × $\frac{7}{16}$	15 0	2·47	6,210	322
" " . . .	3 × 3 × $\frac{7}{16}$	8 0	2·09	19,569	158
" " . . .	1 × 1 × ⅛	5 0	0·23	6,838	300
Flat-end L-iron . . .	3 × 3 × ⅜	8 0	2·09	19,569	158
" T " . . .	3 × 3 × ⅜	8 0	2·55	19,020	175
Hinge-end L " . . .	3 × 3 × ⅜	8 0	2·11	13,199	164
" T " . . .	3 × 3 × ⅜	8 0	2·47	13,117	161
Fixed-end L " . . .	3 × 3 × ⅜	7 6	2·04	19,461	147
Flat-end L-iron . . .	1 × 1 × ⅛	5 0	0·23	6,838	300
" T " . . .	1 × 1 × ⅛	5 0	0·30	6,333	231
	Inches Dia.				
Welded tubes, flat ended .	2·87	15 0	1·64	14,810	190
" " " .	2·87	8 0	1·64	20,122	101
" " " .	2·37	15 0	1·04	16,680	221
" " " .	2·37	8 0	1·08	27,780	123

The quality of iron experimented with had an average ultimate tensile strength of 49,000 lbs. per square inch, with the elastic limit 32,000 lbs., and 18 per cent. of elongation in 8 inches.

D. K. C.

On the Employment of Mathematical Curves as the Intrados of Arches. By WM. H. BOOTH.

(Van Nostrand's Engineering Magazine, vol. xxx., May, 1884, p. 353.)

The Author deals with the design of tunnels, using the formulas given in Rankine's "Civil Engineering," and shows how these formulas may be applied in practice, and how certain difficulties which Rankine does not very clearly elucidate may be surmounted. The method employed is that of "transformation," the tunnel or geostatic arch being derived by a parallel projection from the hydrostatic arch according to the nature of the earth passed through, the widths of hydrostatic arch being shortened in the ratio

$$c = \sqrt{\frac{1 - \sin \phi}{1 + \sin \phi}},$$

ϕ being the angle of repose of the earth.

In thus designing a tunnel there are usually given as data: the depth of the crown of the tunnel below the surface $= x_0$; the tunnel half width $= s$, and the height from crown to invert h. Knowing these and the angle ϕ there remain to be found: the depth of the tunnel's greatest breadth below the surface $= x_1$; the radius at the crown $= r_0$; the radius at the depth, $x_1 = r_1$, and the hydrostatic half span y of the arch by whose transformation the tunnel is to be designed.

The first operation is to find $c =$ the ratio $\frac{s}{y}$ in which the hydrostatic widths are to be reduced to give the tunnel widths.

This would be simple enough but that the reduced width might not exactly coincide with the required breadth of tunnel, and the object is therefore to find at what depth x_1 below the surface the dimension s may be found to agree with the theoretical requirements. The dimension wanted is $a = x_1 - x_0$, that is to say, the rise of the intrados of the tunnel above its greatest horizontal diameter, below which diameter the tunnel walls slope inwards again.

As a first approximation a is made equal to $b \times \frac{s}{y}$, and using Rankine's formulas $b = \left(y + \frac{y^2}{30\,a}\right)$, a value of b is obtained with the quantity a as a factor in the denominator of the fraction.

Another value of b is found thus:—$b = a \sqrt[3]{\left(\frac{x_0 + a}{x_0}\right)}$ when the factor a falls in the numerator.

If a has been assumed too small, the first result gives b too large and the second too small, and *vice versâ*; and a second choice may be made, or b may be taken as the mean of the two results.

Rankine also gives $y = b - \frac{b^2}{30\,a}$, and thus the true value of a may soon be found a very near approximation, being a mean value between $h \times c$ and y. The figure of the hydrostatic arch being drawn by an approximation of circular arcs, we have $r_0 = a + \frac{a^2}{2\,x_0}$ and $r_1 = a - \frac{a^2}{2\,(x_0 + a)}$, and the tunnel is then to be drawn by reducing the breadths in the ratio c; the radii at the crown and springing of the tunnel being $r_0\,c^2$ and $\frac{r_1}{c}$.

Methods of finding the locus of centres for close approximations to the true form are given.

The importance of considering true forms for arches not deeply buried is shown by the rapid increase in the value of the quantity $\sqrt[3]{\frac{x_0 + a}{x_0}}$, as the crown approaches the surface of the ground, this quantity evidently varying from unity at an infinite depth to infinity at the surface.

Where the value is unity, the figure of a tunnel is clearly that of an ellipse, just so much of the ellipse being taken as will meet the requirements of the case, the minor axis k being the tunnel breadth, below which sufficient of the major axis is taken to satisfy the term h, the lower part of the ellipse being discarded, and the tunnel walls held from crushing inwards by the invert. In the design of an elliptic tunnel, the half span s having been fixed, the major semi-axis will be $\frac{s}{c}$; c being, as before, the coefficient of horizontal contraction dependent upon the angle of repose ϕ of the material. The ellipse, however, is only suited as a tunnel section when $\frac{x_0}{x_0 + a}$ is practically equal to unity.

W. H. B.

The Garabit Viaduct.

(Les Annales des Travaux Publics, 1884, vol. v., p. 1130.)

The above viaduct carries the line from Marvejols to Neussargues, near the town of Saint Flour, across the deep valley of the Truyère, in the Cantal department, France, at a height of 420 feet above low-water mark. To avoid giving the central piers of the viaduct too great a height, the deepest part of the valley was spanned by a single arch of 541 feet 4 inches width, and 213 feet 3 inches mean rise. Thus the two highest piers adjoining the arch rise only to 262 feet 6 inches, a height which is still within the limits of those already existing.

The Garabit viaduct is similar in construction to that over the

Douro, near Oporto, by the same designer.[1] It is formed of a large crescent-shaped arch carrying the superstructure over the central part of the valley, continued on one side by four, and on the other side by two large lattice-work spans, these resting on iron piers. A series of smaller spans on masonry piers complete the viaduct on both sides.

The arch is composed of two main ribs of lattice-work, having a clear span of 541 feet 4 inches, and a rise of 196 feet 10 inches at the intrados. It bears on hinges at the abutments which form also the bases of the adjoining piers; its depth at the crown is 32 feet 10 inches, and the shape of the crescent is such that its mean line follows, as nearly as possible, the parabola of the curve of pressure. Transversely the two ribs are strongly braced together and lean towards one another, their distance apart being 65 feet 7 inches at the springings, and only 20 feet 6 inches at the crown. The superstructure takes four bearing-points on the arch, these being symmetrically situated with regard to the centre of the arch, and 80 feet 10 inches apart from centre to centre. It is composed of iron girders of the single lattice type, 16 feet 11 inches deep, disposed in three sets of continuous girders—one over the central portion of the arch, and the two others over the side spans and the springings. The road is carried at 5 feet 6 inches below the top flange of the main girders, the upper part of these serving as a parapet.

The piers are formed of a masonry base supporting an ironwork pier, formed of four columns slightly inclined towards one another, and tied together by horizontal and vertical bracing. Their mode of construction is different from that adopted in the Douro viaduct. In the latter they have the shape of independent box girders tied by the bracing; in the present structure they are U-formed, and each two columns, situated in the same tranverse plane, form, with the bracing, a complete girder, the columns acting as flanges and the bracing as web. The latter is double, and all struts are stiffened by transverse diagonal bracing. Hence the sides of the piers form complete girders, which resist much better the pressure of the wind. The two largest piers, those next the arch, have a total height of 262 feet 6 inches; of these 200 feet 8 inches come on the ironwork, and the remainder on the masonry. Transversely these piers measure 49 feet 3 inches at the base, and 16 feet 5 inches at the top; the batter being approximately 1 in 12. Longitudinally the cross-dimensions are 23 feet at the base, and 7 feet 8 inches at the top.

The total length of the viaduct is 1,715 feet 2 inches, and that of the main portion above described 1,470 feet 10 inches. This is made up of—

	Feet.	Ins.
Two spans of 169 feet 10 inches each, and three spans of 183 feet each on the Marvejols side	888	8
Three spans of 80 feet 10 inches each over crown of arch .	242	6
Two spans of 169 feet 10 inches, on the Neussargues side .	339	8

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[1] Minutes of Proceedings Inst. C.E., vol. li., p. 302.

The mode of erection was that adopted at the Douro viaduct. The great height above the ground rendered timber staging and centering much too costly. The two continuous girders forming the superstructure on each side of the arch were first rolled into their place, after completion of the piers. Their ends on the abutment side were then solidly anchored to the masonry by means of steel cables, whilst the other ends projected by a certain length over the two piers near the arch. The ribs were then built up from the springings, the first bay being put together on a light staging resting on the masonry base. This bay was suspended by means of cables to the main girders over their bearing on the adjoining piers. Two derricks placed on the end of the erected portion served to raise into their place the parts of the following bay, which when put together and braced with the corresponding part of the second main rib, served in its turn as support to the derricks. Each cantilever thus formed was gradually lengthened until joining in the centre, the point of attachment of the cable varying with the length of the cantilevers. The cables had a diameter of $1\frac{1}{16}$ inch, and their greatest strain did not exceed 15 tons, whilst their breaking-load was 80 tons. The erection of the arch was begun in July, 1883, and was completed on the 26th April, 1884; the weight of the iron entering into the arch being 1,200 tons.

The total weight of the ironwork amounts to 3,200 tons, and the cost of the whole work to £124,000; this corresponds to a cost of £223 16*s.* per lineal metre, and, approximately, to £3 4*s.* per square metre covered in elevation.

The viaduct was built under the direction of Messrs. Banby & Boyer, government engineers, by Messrs. Eiffel & Co., of Paris, who also constructed the Douro viaduct.

A. O. B.

The Railway Bridge over the Ems at Weener. A. Böhlk.

(Zeitschrift für Baukunde, vol. vii., 1884, p. 201.)

The Oldenburg State Railway from Ihrhove (on the Hanoverian West Railway) to Neuschanz, the terminus of the Netherlands State Railway, crosses the Ems about $1\frac{1}{4}$ mile east of Weener by a bridge 377 yards long. The agreement with the Prussian government stipulated for three river-openings of 157 feet each, two movable spans of 66 feet each, eight flood-openings of 47 feet, and two of 34 feet each. All the piers were to be built for a single line only, with the exception of those for the swing-spans, which were to be built wide enough for a double line up to high-water mark. The military authorities stipulated for facilities for blowing up two of the river piers.

The foundation, as usual in the river beds of the North Sea basin, is the "Watt-sand," overlaid by from 3 feet to 8 feet of peat and clay. The ordinary height of tide is 5 feet 8 inches, rising to 7 feet 6 inches at springs. If strong east wind coincides with neap tide, the rise is almost imperceptible. A depth of 13 feet below high water (ordinary) was allowed for the river. No special measures were required to protect the bridge from ice.

The foundations consist of brick cylinders, sunk by their own weight and subsequently filled with concrete and brickwork, a method used with the best results in many of the bridges of the Oldenburg State lines, even under more difficult circumstances than those of the Weener bridge. The cylinders (except those for the centre pier of the swing bridge, which were 18 feet 4 inches) were 13 feet 1 inch in external diameter. They were built upon an annular curb, wedge-shaped in section, 1 foot 3 inches wide, and 1 foot 4 inches to 1 foot 8 inches deep, according as the cylinders were built in position upon the ground or suspended from a floating stage. From this sill the brickwork was carried up vertical upon the outer face, and gradually increasing in thickness by half-brick projections on the inside from 1 foot 3 inches, the width of the curb, to its maximum of 2 feet 1 inch. The interior dimensions of the cylinders were to some extent determined by the size of the dredgers used for excavating within them. The thickness of the brickwork was such as to give sufficient weight to sink the cylinder, and to resist the shocks of floating ice during construction.

The pressure upon the foundations was not to be more than 2¾ tons per square foot; it was therefore necessary that the piers on which the ends of the swing-bridge rested, and which were to be built as for a double-line bridge, should be built upon three cylinders, the piers for the large river-spans upon two, and the remaining piers upon one cylinder. The foundation of the west abutment was the only one not built upon these cylinders.

The cylinders, for a height of 8 feet 3 inches from the curb, were built inside a wooden sheathing, and the bolts suspending them from the stage during construction and lowering were built into the brickwork. In form they are circular at the bottom and gradually taper in side elevation towards the top, which is at low-water mark. At this height the ordinary brick pier begins. The mean thickness of the piers and cylinders conforms to the formula

$$d = 3 \cdot 28 + \frac{l}{30}$$

where d is the mean thickness, and l the width of openings to be spanned, both in feet.

The cylinders were built of brickwork in cement (1 of cement to 2 of sand), faced, in the case of the river piers above the bed of the river with alternate courses of whole and half bricks in cement (1 of cement to 1 of sand). When it had been sunk to its proper level the inside of the cylinder was filled with concrete to a height of from 10 feet to 11 feet 6 inches, the first 3 feet 3 inches being in cement, the remainder in "trass." The remainder of the cylinder, up to ordinary low water, was filled with common brickwork in trass mortar (1 of trass, 1 of lime, 2 of sand).

The piers which stand upon these filled-up cylinders are of common brickwork in trass mortar, faced with hard brickwork in cement, in alternate courses of one and one-and-a-half brick. The top of the pier is stepped up, in brickwork in cement, to each of the cast-iron bed-plates, which puts a pressure of 9·1 tons per square foot on the top of the pier, a pressure one-third less than the maximum allowable. At 3 feet below the bed-plate this weight was spread over the whole cross-section of the pier, reducing the pressure per square foot to 2 tons.

The cylinders for the river piers were sunk from a floating stage carried by two barges, between which there was just room enough for the cylinder. They were hung by bolts and links from a circular timber frame, and were sunk on the ebb tide. Details of the stage and framing are given in the plates. It took, on the average, four days to build and sink a cylinder to the bed of the river. Brickwork and excavation were then carried on by turns. The sinking was, as a rule, done without any pumping, this being only resorted to when, owing to the stiffness of the foundation, the weight of the cylinder was insufficient to carry it down. Only in one pier (No. 9) was any serious impediment met with. About half-way down there was a layer of old oak stems, four of which lay across the edge of the cylinder, and had to be cut through under water by means of an axe blade attached to the monkey of a pile-driver.

The excavation inside the cylinders was done by steam-dredger, hand-dredger, and an "Indian" scoop. This last, which was 2 feet 1 inch by 1 foot 7 inches by 7 inches, was attached by a hinge to a vertical guiding rod, and was dropped, edge first, into the material to be dredged, being then pressed down by a man standing upon a step attached to the rod. It was then wound up by a crab. By one hundred and forty lifts from a depth of 13 feet it excavated 9 cubic yards a day, the cylinder in the meantime sinking 1 foot 8 inches. Although the steam-dredger was somewhat cheaper in its work per cubic yard, the scoop was preferred because it excavated round the edges of the cylinder so much better, and the cylinders consequently sank more evenly than when the dredger was used. In the large cylinders for the swing bridge, a steam-dredger and two scoops were worked together, excavating 15¾ cubic yards per day, at a cost of 4*s.* 1*d.* per cubic yard.

The average cost of the whole of the piers, including cylinder, concrete and brickwork, was 45*s.* 1*d.* per cubic yard. The large piers cost 32*s.* 10*d.* per cubic yard. The average is brought up by the large proportion (100 cubic yards out of a total of 125 cubic yards) of the work in the smaller piers, which is below low water.

No details are given of the superstructure. The girders are braced girders of ordinary patterns; those of the small spans have parallel flanges, of the main river-spans horizontal bottom flanges and parabolic top flanges, this being reversed in the swing-bridge. All the bed-plates of the small spans are fixed, those of the large spans alternately fixed and rolling (on a single roller). The swing-bridge hangs upon a pivot about 5 feet from the centre of the pier, so that when the bridge comes to be doubled (the additional swing-bridge being pivoted 5 feet to the opposite side of the pier), the two bridges may be opened at the same time without interfering with one another.

The annexed Table gives particulars of the loads and weights:—

—	L.	K.	*p*.	G.
Main openings	161·81	24·49	12·24	67·91
Swing Bridge, 2 openings together	131·24	27·55	9·18	51·42
Flood openings	49·28	27·55	9·18	9·99
„ „	36·97	29·38	7·34	5·95

Where L is the span in feet, K the moving load in cwts. per lineal foot single line, *p* the dead load in cwts. per foot run, G the total weight of wrought iron in the span in tons.

The weight of iron in one main opening is made up as follows:—

	Tons.
Two main girders	49·46
Cross girders	5·16
Longitudinal bearers	9·85
Cross bracing	3·44
	67·91

The wrought iron is strained to 4·77 tons per square inch.

The total weight of iron in the bridge is 364 tons.

The total cost of the bridge, including superintendence and the maintenance of the plant, which for the most part had not to be purchased, was £26,000.

W. B. W.

The Arlberg Railway.

(Wochenschrift des österreichischen Ingenieur- und Architekten-Vereins. 21st June, 1884.)

At the beginning of June the Austrian Association of Engineers and Architects paid a visit to the Arlberg line, which is to unite Innsbrück and Bregenz. The line has been open since July, 1883, as far as Landeck, where the visitors began their real inspection of the railway with the almost completed bridge over the River

Inn. Here they had their first opportunity of seeing the ashlar-work, which has been used much more extensively for piers and arches on the Arlberg line than on any previous railway undertakings under State control. The ironwork of the main span (196 feet 10 inches) consists of parabolic girders turned downwards, with stiff diagonals and the rails on the top.

Up to Pians the line runs along a wet slope on the south side of the valley, and is not specially interesting. The drainage-works on this part seem innumerable. After this the mountain characteristics begin. Dry walls of considerable height (up to 80 and 100 feet) carry the line along rocky declivities of mica and clay-slate; in some places these are replaced by viaducts built against the cliffs. The extensive use of stonework and walling calls to mind the Semmering line, but there the long lengths of dry walls and masonry walls are unknown. The style of the Arlberg line may be said to hold a middle place between those of the Semmering and Brenner.

After the great Trisana viaduct comes the most difficult part on the eastern side. Close, one after another, come two tunnels and two aqueducts; then two retaining-walls, stone pitching and dry walls. The last had in some cases, where the solid rock could not be reached, to be founded in the "screes."

At St. Anton the tunnel begins; the permanent way, laid with iron cross-sleepers on Heindl's system, is completed, and only some small alterations require to be made here and there in the arch.

Between Langen and Bratz extraordinary difficulties had to be overcome. Here are long stretches of retaining-wall, and important bridges and viaducts, made necessary by the innumerable torrents. The line is a considerable height (up to 390 feet) above the bottom of the valley, so that the bringing up of materials had to be done by lifts, of which four had to be erected on the east and seven on the west side. The first important object on this length is the beautiful bridge over the Wäldlitobel,[1] a segmental arch, crossing a rocky gorge 160 feet deep.

After the Höllentobel viaduct, which consists of three semi-circular arches, each of 72 feet 2 inches span, come two short tunnels, and then the Schmiedtobel viaduct, which, except for the Trisana viaduct,[2] is the largest bridge on the Arlberg line; it consists of three arches of 72 feet 2 inches span each, and two of 39 feet 4 inches each, while the piers are 187 feet high from the base to the springing of the arch. It is followed by several tunnels, of which the longest is 204 yards in length. This part of the line, where it is led through and along precipices which fall some 330 feet sheer down to the bottom of the valley, is without doubt the most interesting technically, as well as the most beautiful.

W. B. W.

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[1] *Post*, p. 405.
[2] Minutes of Proceedings Inst. C.E., vol. lxxvi., p. 410.

The Wäldlitobel Bridge on the Arlberg Railway.

By K. KINZER.

(Centralblatt der Bauverwaltung, 1884, p. 249.)

This bridge, situated about a mile and a half from the entrance to the great tunnel, carries the Arlberg Railway across the Wäldlitobel. It is not so much remarkable in design as in the method adopted in the construction.

The bridge, which is entirely of masonry, lies on a gradient of 1 in 32·85, and consists of one segmental arch of 134·5 feet span, with a versed sine of 43·4 feet, and a small semicircular arch of 26 feet span required in one of the abutments by the nature of the section of the valley; in order to keep the structure as light as possible, four semicircular arches (6·5 feet span) with piers, each containing a jack arch, were inserted in either of the two spandrils. The width of the bridge between parapets measures 14·8 feet, the thickness of the arch is 5·6 feet at the crown, increasing to 10·2 feet at the springing. The centering was a work of considerable magnitude, and consisted of five ribs placed 4·5 feet apart, each resting on nine supports securely braced together; two of these supports were sustained by a truss spanning the deepest portion of the gorge, which measures some 200 feet below rail-level.

The chief novelty in the mode of construction, and one which had previously never been introduced in either Germany or Austria, lay in commencing the construction of the arch simultaneously at four points, two of which being as usual at the springings, whilst the other two were situated midway between the springing and crown. The latter points were supported by a series of powerful timber struts, well braced together, and abutting against the slope of the rock. The arch was keyed in at the crown in the usual manner; the remaining two keyings, however, required very careful work, so as to avoid all settlement liable to occur in removing the timber abutments.

The object in turning the arch in the above manner, was to ensure equal loading of the centering, more rapid and perfect setting of the mortar, and greater despatch in the building of the whole.

The total cost of the bridge amounted to £3,000, the centering costing £830.

J. R. B.

Improvements in the Permanent Way and Works of Italian Railways. By F. BIGLIA.

(Giornale del Genio Civile, 1884, p. 105.)

This Paper contains the results of the second of the three commissions upon matters relating to the Italian railways referred to in the Abstract on p. 411. The Berne International Conference, mentioned in that Abstract, recommended that the international gauge should be from 4 feet 8·5 inches to 4 feet 8·69 inches. This was objected to by the French and Italian delegates, because in France many of the principal lines have a gauge 4 feet 9·08 inches, and in Italy the gauge is fixed at 4 feet 8·89 inches, a mean between the French of 4 feet 9·08 inches, and the German of 4 feet 8·5 inches. The Italian Commission agreed to accept the gauge proposed at Berne as soon as it should be adopted by the French Government.

Foot Signals at Points.—The Commission decided that these are not to be considered as stopping signals, their object being to enable the station-men to see from a distance how the points stand, and to show the engine-driver upon what line he is travelling. There was a long discussion as to the colour to be adopted for these, as red ought to be used exclusively for stop signals, but nothing was decided. These signals are to be limited to the points at the ends of stations, and to those giving access to large groups of goods sidings.

Level-crossing Barriers worked from a Distance.—The following are the decisions of the Commission in regard to these. A bell is to be fixed near each barrier, and rung before it is moved, to give notice to travellers. Barriers must be visible from the points at which they are worked, which must not be at a greater distance than 1,600 feet. Lights are not to be provided, except in special places. One man is not to have more than two distant barriers under his charge, in addition to his own gate, and the levers must be within 32 feet of each other. The barriers are to be placed parallel to the line, at a distance of 6 feet 6 inches from the nearest rail; there is to be a separate lever for each side of the line, and the barriers are to be constructed so that they cannot be opened and shut by passers by. They are to be weighted so as to close gradually in the event of the connection with the lever breaking. The use of these barriers is to be confined to unimportant roads and to single lines of railway upon which the traffic receipts are less than 35,000 lire per kilometre per annum.

Three-throw Points.—It was decided to allow these to be used, but not upon lines traversed by express trains; the central line must be straight. They may also be used on express lines at stations where all trains stop. A system of double points, by means of which trains running in either direction upon either of two lines of way, can either keep their own line or be shunted on to the other, is permitted to be used in certain cases, but limited to slow lines and sidings.

Attachment of Trolleys to Trains.—One of the questions discussed was whether trolleys might be attached to trains by slip-couplings. It was objected that the attachment being necessarily above the centre of gravity of the trolley, there is a tendency to turn it in a vertical plane round the front axle whenever this encounters the slightest resistance. On the other hand the plan is most useful for drawing heavy loads up steep inclines, for when the workmen have to push the trolleys themselves, they arrive at the place where they are going to work exhausted, and, which is worse, in a state of heat which exposes them to attacks of fever in unhealthy districts. The Commission decided to allow the attachment to slow trains on lines having gradients steeper than 1 in 100.

Traffic at Level Crossings.—On the Italian lines there is generally a level crossing at each station, and as these are situated in populous places, they are the occasion of constant complaints from the inhabitants, owing to the length of time during which the gates are closed, either on account of the trains being late or during shunting operations. There are also frequent crossings in the open country. If it is proposed to substitute a bridge for a crossing, the necessary approach is objected to, especially in level districts, when the necessary gradient would limit the loads which animals can draw. Besides this, owing to the station-sidings, it would often be necessary to carry the road across the line at some distance from the station, and this would necessitate a long diversion. The time during which the gates are closed can be limited by the use of signals, but, on the other hand, it is impossible to run quick trains when the driver has always to be ready to stop at these signals. After some discussion, the Commission decided (1st) that in order to ensure sufficient protection of level crossings at which it is important to limit the time of closing the gates as much as possible, they might be entered in the time-tables and treated like stations, and provided with disk signals and electric instruments to give notice when the trains are late; (2nd) that at less important crossings it will be sufficient to provide telegraph communication with the nearest station; (3rd) that on main roads crossing lines of heavy traffic, especially where the roads have steam trams on them, it is essential that bridges over or under the railway should be built, and, considering the interests involved, the cost may be divided between the railway and the public bodies which would benefit by the change. It must not be forgotten that the cost of gate-keepers will be saved.

Ballast.—Ever since the construction of railways was commenced it has been a disputed point whether the ballast should cover the sleepers or be laid level with their upper surface; the former system protects the timber from the direct rays of the sun, from sudden changes of temperature, from moisture, dryness, and frost, and gives excellent results in preserving the sleepers from decay. These advantages are, however, denied by some engineers, who consider that covering the sleepers makes them decay more rapidly, and increases the first cost of the line and the difficulties of maintenance.

The Commission came to the conclusion that though uncovered sleepers may be preferable in northern climates, it is necessary in Italy to completely imbed them in the ballast.

Working strains allowed in Iron Roofs and Cranes.—The working strain allowed in bridges in Italy is 3·81 tons per square inch, provided that the breaking-strain is not less than 20·32 tons, and the limit of elasticity 9·52 tons per square inch. Italy being entirely dependent upon foreigners for supplies of iron, it is found more economical to use Belgian iron, which will stand these tests, rather than stronger but more expensive material. For roofs the Commission fixed upon working-strain, 5·08 tons; breaking-strain, 22·86 tons per square inch, with 8 per cent. extension. Various methods have been suggested for limiting the weights lifted by cranes, among others Hohenegger's apparatus, which indicates the weight which is being lifted, and automatically prevents the lifting of weight in excess of that which the crane is designed to carry. The Commission determined to conduct trials with this apparatus.

There is annexed to the Paper a report upon lighting carriages with oil lamps.

W. H. T.

The Lartigue Elevated Mono-rail. By MAURICE L'ÉPINE.

(Annales Industrielles, March 30, 1884, col. 398, &c.)

The system of Mr. Lartigue consists of a single rail supported by **A**-form iron frames above the level of the ground, provided with panniers constructed with angle-iron, for carrying the motive power and the freight. The rail is a flat bar, about 3⅛ inches high and 0·4 inch thick, having two fillets, one on each side, half way up, giving the rail the form of a cross, of which the horizontal member is extremely short. The rail is comparatively stiff vertically, and flexible horizontally, and is well adapted for laying in an open country. For facility of laying, the rails are in lengths of 3 metres, or about 10 feet, weighing 33 lbs., or 10 lbs. per yard. They are bolted between the upper ends of the **A**-frames, each of which weighs about 11 lbs. The rails are jointed end to end by fish-plates riveted to them. The rail, when fixed, stands about 3 feet above the level of the ground. The panniers hang about 1 foot clear of the ground; they are about 3 feet long, and occupy transversely a width of 5 feet over all.

The rail can be laid without the aid of skilled labour. In an unsettled country, the material is brought to the point of departure, and is conveyed in panniers on the rail itself to the spot for erection. A kilometer, or two-thirds of a mile, per day, can be easily laid by a gang of six men.

The extension of the Lartigue system has been retarded hitherto by the want of a motor, for if it is to be raised high enough to clear the traffic at level crossings, horse-power is not feasible. But the system of the "mono-rail," such as it is, may be serviceable in agricultural and industrial enterprises. A mule can draw a useful load of from 150 to 250 tons, one mile, per day; that is to say, it takes from 12½ to 20 tons 12½ miles, returning empty. The cost for horse-power and driver to draw 1 ton one mile is estimated at 0·17*d.*

Mr. Eugène Bethouard, an agriculturist at Saint-Jean-lès-Rue (Somme), employs the Lartigue mono-rail for the unloading of beet-root, the cost of doing which is about 5*d.* per ton. Formerly, the cost amounted to from 1*s.* 3*d.* to 1*s.* 8*d.* per ton. The operations comprise the laying and readjusting of the way as the work of unloading proceeds; filling the baskets and placing them on the panniers; drawing the train a distance averaging 270 yards; emptying the baskets and replacing them on the panniers; taking the train back for the same distance.

The cost of laying the mono-rail, 32 inches high, comprising a stock of panniers, is £383 per mile. Upwards of 60 miles of the mono-rail are laid in Algeria for the collection of alfa, giving much satisfaction. Many other lines are in preparation for the Colonies.

The Lartigue system has been worked by electric power at the Agricultural Exhibition in Paris. A Siemens' dynamo-electric machine of the horizontal type is fixed on one side of the pannier, and makes 1,200 revolutions per minute; and by means of speed-pulleys driven by ropes, the speed of the driving pulley on the rail is at the rate of 150 revolutions per minute, resulting in a forward speed of about 6 miles per hour. The driver is seated on the other side of the pannier, and has the commutators, brakes, &c., within reach.

D. K. C.

Wear of Steel Rails. By — COÜARD.

(Revue Générale des Chemins de fer, March 1884, p. 105.)

A great number of measurements have been taken of the wear of steel rails on the Paris-Lyons-Mediterranean Railway since 1874, on straight and level lines, inclines, and curves, and in stations and tunnels. The present article is devoted to the consideration of the results of observations on the wear of steel rails on straight and level sections of the way. Rails delivered from seven different steel works had been laid, and from the results of their wear it appears that the rate of wear of rails of the same manufacture was nearly the same for various sections of way. The following are the general results for successive periods of service:—

	Vertical Wear per 100,000 Trains.			Trains per Millimetre of Wear ($\frac{1}{25}$ inch).
1877–80	0·922	millimetre, or	0·0363 inch	108,400 trains.
1880–81	0·927	,, ,,	0·0365 ,,	107,500 ,,
1877–81	0·914	,, ,,	0·0360 ,,	109,500 ,,

It is deduced that, in round numbers, the wear of the rails is at the rate of 1 millimetre, or $\frac{1}{25}$-inch, per one hundred and ten thousand trains. The gross tonnage-weight of trains passed over the rails has also been recorded, and it appears that the wear is at the rate of 1 millimetre, or $\frac{1}{25}$ inch, per 28,200,000 tonnes, or about 27,800,000 tons.

On the question whether the gross tonnage, as is usually accepted, or the number of trains, is the preferable measure for the regular wear of rails, the Author shows from the tables of wear of rails that the coefficient of wear varies much more widely, in terms of gross tonnage, than when taken in terms of number of trains, and he concludes that the number of trains is the preferable measure. He adduces evidence in support of this conclusion from the wear of iron rails on different sections of the line, of various traffic, and also from the Paper of Mr. Price Williams "On the Economy of Railway Working," read at the Institution of Mechanical Engineers.[1]

For an assumed total wear of 10 millimetres, or about $\frac{2}{5}$ inch, the corresponding number of trains is one million one hundred thousand trains.

It appears, from the tables of wear, that the coefficient of wear diminishes with the wear; that is, that the rails wear more slowly for the same traffic, after having been laid for some years. For instance, on the section of way between Melun and Bois-le-roi, in the course of the period 1874 to 1883, the number of trains per millimetre of wear increases from twenty-nine thousand six hundred to seventy-seven thousand five hundred trains. The decreasing rate of wear here signified is explained, no doubt, by the wearing down of the upper part of the rail into a flatter section, and the increasing width of surface subject to wear.

It is remarkable that the wear of the outer rail of each line of way is more rapid than that of the inner rail, to the extent of from 20 per cent. to 30 per cent. There is a corresponding difference against the outer rail, in the percentage of fractures of rails. These differences are ascribed to the less efficient support of the sleepers at the outer sides of the way than in the central portion, between the two lines of way.

The durability of rails made of Bessemer steel is shown to be much superior to the rails of Martin steel. The hardest rails are found to endure the longest.

D. K. C.

Rack-Railways.

(Annales Industrielles, April 20, 1884, column 482.)

Rack-railways, of which the Rigi line was the first application, made by Mr. Riggenbach, have been considerably extended in use. Those which are worked exclusively by locomotives and rack-gear are six in number, as follows:—

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[1] Institution of Mechanical Engineers, Proceedings 1879, p. 96.

	Constructed.	Ruling Gradient.
1. Vitznau-Rigi	1870	25 per 100, or 1 in 4
2. Kahlenberg-Vienna	1872	10 „ „ 1 in 10
3. Schwabenberg-Pesth	1872	10 „ „ 1 in 10
4. Arth-Rigi	1874	21 „ „ 1 in 4·8
5. Rio-de-Janeiro	1882	15 „ „ 1 in 6·7
6. Drachenfels to the Rhine	1883	22 „ „ 1 in 4·5

These are tourist lines employed exclusively in passenger traffic.

The following lines, six in number, are worked on the "mixed" system, partly by rack and partly by ordinary adhesion, for which alternatives the locomotives are purposely constructed:—

	Constructed.	Ruling Gradient.
7. Ostermundingen-Berne	1870	10 per 100, or 1 in 10
8. Rorschach-Heiden	1874	9 „ „ 1 in 11
9. Wasseralfingen	1876	8 „ „ 1 in 12·5
10. Rüti-Zurich	1877	10 „ „ 1 in 10
11. Laufen-Berne	1878	6 „ „ 1 in 16·7
12. Oberlahnstein	1880	10 „ „ 1 in 10

There are 42 locomotives, in all, weighing from 9 tons to 18 tons.

In addition to the foregoing twelve lines worked by locomotives, Mr. Riggenbach has constructed four funicular or rope-railways inclined planes, on which the function of the rack is to act as a security-brake; and where the power is generated by means of water stored in tanks, and serving both as a motor and as a counterweight. The lines are those of Giessbach on the Brienz Lake, incline 28 per 100, or 1 in 3·6; of Dom-Jesus-de-Braga, in Portugal, incline 45 per 100, or 1 in 2·2, opened in 1883; and the Montreux-Geyon line, lake of Geneva, incline 57 per 100, or 1 in 1·8, also opened in 1883.

D. K. C.

Improvements in the Rolling Stock of Italian Railways.

By F. Biglia.

(Giornale del Genio Civile, Jan. 1884, p. 3.)

In the autumn of 1882 the Author was instructed by the Minister of Public Works to reorganise the several technical Commissions which at various times since 1877 had been engaged in improving and unifying the types of rolling stock and permanent way of the Italian railways. There were three commissions, one engaged upon matters relating to rolling stock, another on those connected with permanent way, the third on the best means of lighting trains with oil. The present paper deals with the first subject.

On the best means of providing for the carriage of wounded soldiers in time of war in ordinary carriages.—It was decided to construct a certain number of third-class carriages on the American system, so that the end platforms should form an open space at least

7 feet 6 inches between two adjacent carriages, the hand-rails at the ends being removable. The doors to be not less than 2 feet 8 inches wide.

Continuous brakes.—The Westinghouse automatic compressed-air and the Smith-Hardy non-automatic vacuum-brake have been introduced into Italy, the former on the Turin-Paris line, the latter upon lines where it was considered expedient to increase the speed of the trains by pulling up short at the numerous stations, but where it was not necessary that the brakes should be automatic. There was a lengthy controversy upon the merits of the two systems. The advantages of the former are that in case of any accident its automatic action offers guarantees for security which cannot otherwise be obtained; if the engine leaves the rails, or breaks down, the instant application of the brake keeps the carriages on the line in the former case, and prevents them from mounting on to one another in the latter. It also prevents the carriages from running away in the event of a coupling breaking. It is true that the brake occasionally comes into action accidentally, but the results of this can seldom, if ever, be disastrous. It is also lighter and less bulky than the Smith-Hardy.

It is objected to it, however, that the automatic action is attained only by a sacrifice of that simplicity and economy which are essential to the proper working of a continuous brake.

The advocates of the Westinghouse brake therefore have recommended its adoption on express trains travelling on the principal lines at high speed, particularly those with heavy gradients, and this only gradually, as both the technical staff and the public have to learn the merits and defects of the system.

Communication between passengers and guards.—Several systems were discussed. The best was that of Commander Ubicini, which is an electro-magnetic apparatus in communication with three bells, one on the engine and the others in the guards' vans. At first these were connected by continuous wire, but as this did not admit of the ready making up of trains, the inventor devised a coupling which answered well both as to conductivity, simplicity, and strength. Siemens telephones were tried, and answered fairly well in slow trains, but in quick trains they could not be heard. None of these systems, however, were found to give complete satisfaction, and the Commission were not on the whole favourable to providing passengers with the means of stopping trains. The Minister of Public Works, however, urged the necessity of protecting passengers in some way from personal assaults, and as it was known that any mode which interfered with privacy would be objectionable, it was decided to try the plan of cutting holes between the compartments, and placing thin metal plates in these holes, so that noises could be easily heard between one compartment and another. The cord system was also to be attached to the train on the supposition that if cries proceeded from one part of a carriage, passengers in another would hear them, and procure assistance by pulling the cord.

Means of ascertaining the actual running-speed of trains.—Many instruments have been devised for recording speed, but they all depend upon clockwork which is liable to get out of order from the vibration of the engine. The Commission therefore determined, instead of relying upon such instruments, to have electric signals erected at particular points of the lines, by means of which the times at which the trains passed these signals would be recorded at the nearest station; and thus a check would be kept upon the engine-drivers, especially on heavy falling gradients, where they like to make up time and economize fuel.

Means of heating carriages.—Long discussions were held upon this point. The system at present in use is that of hot-water foot-warmers, the objections to which are—1st, that they have very little effect in raising the temperature of the carriages; 2nd, that the frequent changes necessary cause great inconvenience to the passengers, especially at night, and involve a great amount of work at the stations where the changes are made.

In Germany the carriages are warmed by steam conveyed by pipes either from the engine or the luggage vans. This system is very costly, and requires careful attention to see that the condensed steam is discharged, and it increases the difficulty of coupling and uncoupling the carriages. The heat too is very dry unless the further expense of providing water pans in each compartment is incurred. The system of stoves with hot air and water-pipes is dangerous, and complicates the construction of the carriages. The Ancelin system consists of foot-warmers filled with acetate of soda. These have the advantage over hot-water warmers that they retain the heat much longer, and consequently have not to be changed as often. Several ways of filling the hot-water cans were discussed; that in ordinary use is to fill them with hot water; another is to fill them with cold water, and heat a number together by steam; another to introduce a truck load into a cistern of hot water, which is a rapid and economical method of heating them. An objection to the acetate of soda is that the temperature at first is 140°, but after a time it is reduced to 102°, at which it remains nearly constant.

The final conclusions of the Commission were—1st, for carriages divided into compartments to keep to the hot-water system, heating the cans by steam at the larger, and filling them with hot water at the smaller stations, and at the same time to institute trials of Ancelin's method; 2nd, for carriages having only one compartment, mail vans, saloons, and in local trains, to adopt the system of external stoves with hot-air or water-pipes.

Many other subjects were discussed; in regard to some of them the Commission came to no definite conclusions, others were matters interesting only in regard to the particular requirements of the Italian railway system.

W. H. T.

On the Cleansing of Aqueducts.

By D. FITZGERALD, Boston Waterworks.

(Engineering News, New York.)

With the exception of a small portion, the City of Boston is supplied with water by two aqueducts from Lake Cochituate and from the Sudbury River; the former aqueduct is egg-shaped, with the larger end downwards, and is 6 feet 4 inches high by 5 feet wide, the latter 7 feet 8 inches high by 9 feet wide, with a semi-circular arch, curved sides, and a flat segmental invert.

The Cochituate aqueduct is 14·3 miles in length, with a fall of 3½ inches per mile, and is found to become foul for a distance of 2 miles from the lake by the profuse growth of *spongilla lacustris*, and lower down by slimy vegetable matter. Stiff corn brooms are used for cleaning the roof and sides, and rattan stable brooms for the invert. The whole length is cleaned in three days by gangs of six or seven men, each of whom sweeps 10,600 square feet of brick surface three times over, or 620 lineal feet on conduit. The cost for labour, one hundred and twenty-three days at $2 per day is $246, which, with the cost of candles, brooms and sundries, brings the total expense up to $353.

The Sudbury aqueduct is 15·6 miles in length, with a fall of 1 foot per mile; in cleaning this conduit, 8,350 square feet of brick surface swept three times over, or 580 lineal feet, is found to be an average day's work per man, the sweeping being necessary only up to a foot above the water-line. The cost, including labour (one hundred and thirty-eight days at $2 = $276), amounts to $400. It is intended to clean the aqueducts once in every six months.

A. T. A.

Self-registering Rain-Gauge. A. FRANK.

(Zeitschrift für Baukunde, vol. vii., 1884, p. 217.)

The self-registering rain-gauge is made on the principle of weighing the rain-water, and consists of two parts, the weighing apparatus and the registering apparatus.

From the receiving-vessel the rain-water flows to the collecting-vessel, which, to prevent evaporation, has the smallest possible opening at the top, and is provided with a tap at the bottom to run the water off. A later improvement is to run the water off by means of a syphon, and thus let the receiver be self-acting. The collecting vessel hangs on two wires, which pass over two pairs of pulleys, and are attached to the last of them. To these is fastened a lever, at the end of which, by a hinge, hangs a weight. As the weight of water in the collecting vessel increases, the length of the horizontal arm of the weight increases, so as to maintain equilibrium, the increase in the length of the arm being proportional to the increase in weight of the water. The length of the horizontal arm, which is thus a measure of the amount of rain, is brought to paper as follows. The weight is so hung that it is always vertical, and keeps in a vertical position a light rod which slips through it, and at the end of which is a pencil which records on a strip of paper, passed under it by clockwork at a fixed rate of speed, the length of the horizontal arm. In this way a diagram is made on the paper, its abscissas being proportional to the time and its ordinates to the rainfall.

The weights of the collecting-vessel and of the lever are counterbalanced by weights, and, to diminish friction, all the axles work between steel points.

The defect of the machine at present is, that it does not register snow.

W. B. W.

On Lamaha Water, and a Process for Purifying it.

By E. E. H. Francis.

(Timehri—The Journal of the Royal Agricultural and Commercial Society of British Guiana, Dec. 1883, p. 315.)

The water of the Lamaha Canal is much discoloured by peat. The colouring matter cannot be separated by the mere application of heat, or by the passage of a galvanic current, but it precipitates in brown flocks spontaneously and often completely, when the water is allowed to stand for several months in glass vessels exposed to the light. Nor can the colouring matter be easily destroyed by oxidation. Filtration through animal charcoal partially removes it, and it can be completely separated by acetate of lead, hydrate of iron or alumina. From a gallon of the water there were obtained by precipitation with alumina 4·737 grains of colouring matter, and the water then became perfectly white. The decolourising power of charcoal or spongy iron filters is very limited for Lamaha water, and the filters soon become choked. It was found by experiments that 1·614 grain of alumina would remove the colouring matter from a gallon of water. This quantity of alumina is contained in 15 grains of alum; but as alum is an expensive source of alumina, a cheap substitute was sought for and at last found in "alumino-ferric cake," a neutral sulphate of alumina manufactured by Mr. Spence, of Manchester.

In most cases where alum is used for purifying water, the lime present in the water is sufficient to decompose the aluminous compound, the quantity of lime in Lamaha water is so small (the hardness being 4°), that it is necessary to add lime. The quantities required to remove the colouring matter from one million gallons, are 1,654 lbs. of alumino-ferric cake, and 324¼ lbs. of Bristol lime.

The following are analyses of the water before and after treatment in grains per gallon:—

	Before Treatment.	After Treatment.
Vegetable and organic matter	4·653	0·558
Combined matter	..	1·512
Iron peroxide	0·701	0·070
Alumina	0·230	..
Lime	0·050	2·352[1]
Magnesia	0·133	0·278
Potassa	0·116	0·140
Soda	0·965	1·315
Silica	0·500	0·154
Sulphuric acid	0·432	4·385
Chlorine	0·371	0·479
	8·151	11·243
Deduct oxygen	0·167	0·216
	7·984	11·027

The hardness of the treated water was 5·7 degrees.

The process has not yet been applied to the treatment of the regular water-supply; but it has been tried successfully in a reservoir holding about 25,000 gallons, and it is now proposed to use it in the following way. A channel, about 100 feet long, and 6 to 8 feet wide, is to be cut from the canal to the existing filter-beds. In the first part of this channel, an open wooden box is to be suspended with a few slabs of alumino-ferric cake. By properly adjusting the height of this, the water will dissolve exactly the proper quantity as it flows through the box. Along the remainder of the channel a few hogsheads of lime are to be distributed. If the current is not too rapid, no mechanical disturbance of the lime will take place; but the water will take up the exact quantity necessary to set free the alumina and colouring matter. The water will then pass on to the filter beds, and leaving the impurities on the surface, will pass through white and clear. The sand can be cleaned when necessary by flushing. The water in the channel can be tested by holding violet litmus paper in the channel. When the proper quantities of alumina and lime are present, the water will slightly redden the paper.

W. H. T.

Temperature of Water at various Depths.

By Hamilton Smith, Jun.

(Transactions of the American Society of Civil Engineers, March 1884, p. 73.)

This paper is a summary of available published information on the respective temperatures of water at various depths in lakes and oceans. In the Lake of Geneva observations were made by F. A.

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[1] Equal to 3·108 grains of slaked lime.

Fosel, from May, 1879, to January, 1880, and whilst the surface temperature varied from 41° to 71°·6 Fahrenheit, the temperature at the depth of 984 feet was constantly 41°·4; and at 164 feet, it varied from 43°·3 to 44°·8. This lake rarely freezes. In Lake Tahoe, California, the temperature was 39·2° Fahrenheit at the bottom, at a depth of 1,506 feet, whilst it was 67° at the surface. The "Challenger" surveys showed, that at a depth of over 16,000 feet the temperature at the bottom was from 30°·9 Fahrenheit to 31°·5. The "Blake" observers found, north of St. Thomas, in the West Indies, 36¼° Fahrenheit at a depth of 27,336 feet, whilst at the surface the temperature is nearly constant at about 80°. The Author is of opinion that pressure, directly or indirectly, may be a factor in the producing cause of the differences of temperature. In the deep soundings off St. Thomas, the pressure of the water amounted to nearly 12,000 pounds per square inch.

It was stated in discussion of the paper, that the temperature is practically the same for a depth of 30 feet from the surface of the ocean, the effect of the continual agitation above that level. In Lake Superior, whilst the temperature at the bottom averaged about 39° Fahrenheit, it varied from 50° to 56° Fahrenheit at the surface, for depths to the bottom of from 350 feet to 1,000 feet. In Lake Michigan, cold water is obtained by fishermen by sinking from a boat a corked jug to some depth in the lake, pulling out the cork, and letting the jug fill there.

In response to Mr. R. L. Harris, who commented on the contrast between the decreasing temperature of water, and the increasing temperature of the solid earth with the depth, Mr. E. B. Dorsey stated that the mines on the Comstock Lode, Nevada, were exceptionally hot. At depths of from 1,500 feet to 2,000 feet, the thermometer placed in a newly-drilled hole indicated 130° Fahr. Large bodies of water have run for years at 155° temperature, and smaller volumes at 170°. There have been fatal accidents by falling into this water. The temperature of the air is kept down to 110°, by forcing down fresh air, cooled over ice, from the surface. In sinking the Forman shaft of the Overman mine, which was commenced about 4,000 feet east of the outcrop, entirely clear of the vein, three holes were drilled into the rock, 36 inches deep, at each 100 feet of depth, for a total depth of 2,300 feet. A thermometer was placed in each hole, and plugged up with clay. The following is a selection from the readings of the thermometers at the end of twelve hours:—

Feet.	Fahrenheit.	Increase of Temperature per 100 feet.
100	53°·0	0·33
500	66°·1	
1,000	82°·4	0·30
1,500	98° 6	
2,000	112°·5	
2,300	121°·2	

It is shown that the rate of rise of temperature diminishes for the greater depths. A Table is added showing the temperatures of wells and of water in several of the most noted mines, tunnels, and artesian wells. The temperature of water in mines or artesian wells, or from springs, is not necessarily the temperature of the rocks in the vicinity of its discharge. In the Comstock mine the water is from 30° to 40° Fahrenheit hotter than the adjacent rock; in the St. Gothard Tunnel it is 6° warmer. In hot springs, boiling water is discharged through comparatively cool rocks. According to the Table, the coolest mines are the Chañarcillo mines, in limestone, and the coolest tunnel is the Mont Cenis, also in limestone; whilst the hottest mines are the Comstock, in trachyte, and the South Balgray, near Glasgow, in the coal measures.

D. K. C.

On the Annual Limits of Oscillation of the Water-Level in the Rivers and Lakes of European Russia.

(Ingener, St. Petersburg, 1884, vol. i., p. 116.)

The article has been called forth by the recent appearance, under the auspices of the Ministry of Ways of Communication, of a Paper entitled "Information respecting the Level of Water in the Rivers and Lakes of European Russia." The Paper is divided into the following heads:—

1. Explanatory notes.
2. Instructions for observing the levels of water.
3. A table of gauging stations in operation from 1876 to 1881.
4. Information respecting the periods and corresponding levels of some of the most important annual fluctuations.
5. Information respecting the maximum, mean, and minimum levels for every month.
6. Eighty graphic delineations representing the fluctuations of water-level.
7. A map of European Russia, on a scale of 160 miles to the inch, showing the positions of the eighty gauging stations.

The stations are of two orders. In the first the observations are made all the year round three times a day; in the second, readings are only taken once a day, during the time the waters are free from ice.

Although the number of stations in 1876 was 132, and in 1880 341, information is given respecting only 80 stations, the records of which had been proved to be thoroughly trustworthy.

The curves of variation of water-level are given for the eighty selected stations for the years 1877, 1878, 1879, and 1880.

The following general deductions are made: The smallest variation of level occurs in the lakes, where it is about 2·8 feet, and the largest was observed on the Volga, at Kalooga, where it reached 45·5 feet. The larger part of the rise of the water-level is due to the melting of the winter snows, and causes 1·4 foot rise in the lakes, and 42 feet in the Volga. The fall of water-level due to summer heats is 1·4 foot in the lakes, and 12·6 feet in the Volga at Sarakoff. The variations are found to be smallest at the mouths of the rivers. The author of the Paper, discusses many points of interest connected with the vast quantity of observations recorded, and points out the great value of the information to engineers.

W. A.

Apparatus for Measuring the Velocity of a Stream of Water.

By L. CHENUT.

(L'Électricien, vol. vii., 15th March, 1884, pp. 259.)

This is a brief description of an apparatus devised by Professor Harlacher, and exhibited at the Vienna Exhibition. It consists essentially of a rod of iron 4 metres (13·12 feet) long, and 0·032 metres (1¼ inch) in diameter, with a longitudinal groove 0·005 metre (two-tenths of an inch) broad. A bracket is firmly fixed to the rod near its lower end, and to this a cord is attached, which passes over a pulley carried on the upper end of the rod. The cord serves to raise or lower the apparatus in the water. To firmly fix the rod in the bed of the stream, it is pointed at its extremity and provided with a circular bed-plate a little above the point. The fly has a horizontal axle carried on agate bearings attached to the bracket. It consists of two helicoidal blades, 0·15 metre (2·4 inches) exterior diameter, and 0·05 metre (0·8 inch) interior diameter, and 0·30 metre (4·8 inches) pitch. The axle carries a worm gearing into a worm-wheel. The teeth of the latter press against a platinum strip, arranged so that as each tooth passes, it completes an electric circuit in connection with a battery placed in a convenient place on shore. The whole of this mechanism is shielded by a suitable box. The electrical circuit is in connection with an electro-magnet acting on the anchor of the escapement of clockwork. At each passage of a tooth of the worm-wheel, the anchor is released, and the clockwork records a portion of a revolution. The cord serving to hold the instruments is made up of the conducting wires of the circuit properly insulated. The description is illustrated by a sketch.

E. H.

The Floods of the Saône and its chief Tributaries in 1882.

By — REMISE.

(Annales des Ponts et Chaussées, 6th series, vol. vii., 1884, p. 238, 3 pl.)

A series of floods occurred during the latter half of 1882 in the Saône basin, which it has been possible to study in detail, owing to the establishment, in March 1882, of the system of the measurement and prediction of floods for that river, similar to that organised on the Garonne and other rivers in France. Forty-six hydrometrical stations have been established in the basin, and twenty-three meteorological stations, some of the latter being included in the former. At the former the height of the river is regularly recorded, and notice of the approach of a flood is sent from the secondary stations to the principal ones, and from thence to the riparian inhabitants; and at the latter the rainfall is observed, and information is transmitted of great falls of rain or snow, and the direction of the wind is noted. The Upper Saône comprises the portion of the river between its source and Gray. In July this river and its tributaries were all in flood after the torrential rain of the 7th to the 9th, but they had returned to their normal state in August. Though some floods occurred in the autumn, the next most important one was at the end of December. The Little Saône lies between Gray and Verdun, and its floods were similar to those of the Upper Saône. The confluence of the Doubs marks the separation between the Great and Little Saône. The floods of the Doubs do not always coincide with those of the Saône. In 1882 its floods commenced in May; in the latter half of the year they were almost at the same dates as those of the Saône. On the Upper Doubs the July flood was small, but on both the upper and lower rivers the December flood was the largest of all. The floods of the Great Saône are mainly the result of the combined floods of the Little Saône and the Doubs, as the lower tributaries only assist in maintaining floods already existing. There were six floods of the Great Saône in 1882, which rose above the level at Chalons at which the movable weirs have to be lowered, comprising a total period of ninety-seven days. The first was a very short flood from the Doubs, in June; the second was the great flood of July, which, happening during the hay-harvest, was the most hurtful; next, in September, came a flood from the Doubs, followed by a second rise from a flood of the Saône; then followed a flood, at the end of October, due to both rivers; afterwards a flood lasting from the 9th of November to the 22nd of December, due to several successive floods of the Little Saône and Doubs combined; and, lastly, the great flood of December, higher than any observed since 1856. The cause of the unusual July flood was a widespread excessive rainfall, attaining a depth of 5·8 inches in fifty-six consecutive hours, to which the only known parallel in France was in September 1866, which occasioned an exceptional flood in the basin of the Seine. The rain commenced very early on the 7th of July in the Saône valley, and the Saône began to rise in the evening; it rose very rapidly on the 8th, at the rate of about $\frac{1}{3}$ inch per hour, and on the 9th the inundation commenced everywhere. The flood on the Upper Doubs was small; but on the tributaries along the right bank of the Great Saône, more especially within the area of greatest rainfall, the floods were quite exceptional in their rapid rise and height. The area inundated amounted to about 66,700 acres of meadow, and 27,200 acres of arable land, and the damage was estimated at £160,000.

Much of the land is protected against ordinary summer floods by embankments, but most of these were submerged. The flood of December took place under different conditions to that of July; it was due to a great rainfall from the 25th to the 28th, but this rainfall, though less than in July, was preceded by rains on saturated ground. The Doubs and the Ognon experienced the greatest floods, the Doubs rising more than 1 foot higher than previously recorded; whilst at the confluence of the Doubs and the Saône the flood was barely less than the maximum in 1840. The flood lasted twenty-one days, and the discharge reached 61,000 cubic feet per second at Chalons, and 77,000 cubic feet at Macon. When a flood occurs in the upper river, its rise is compared with previously observed floods; so that at Chalons, and all along the Great Saône, the maximum has been always predicted from three to five days beforehand, and its height determined within from 4 inches to 1 foot of the actual rise.

L. V. H.

Annual Report of the Improvements of South Pass, Mississippi River.[1]

By Capt. W. H. Heuer, U.S.A., 1883.

The report states that there has been throughout the year, with the exception of five days, a channel 30 feet in depth, extending from the jetties to the gulf, having at present a least width of 90 feet, and that the 26-feet channel between the jetties maintained a least width of 200 feet, excepting for nine days, and that dredging has been carried on only on seventeen days.

A new jetty has been constructed in the channel for a length of 6,810 feet, 200 feet within the east jetty and parallel to it, thus reducing the water-way from 1000 feet to 630 feet, but improving the depth.

During the year a cyclone passed over the works, and damaged the east jetty for a length of half a mile, concrete blocks 28 tons in weight being torn away or turned over. This occurred, fortunately, when the new inner jetty had already been constructed, so that no injury was done to the channel. No work has been done upon the jetties themselves within the year, but operations have been confined to the new inner jetty, and to building five new wing-dams from the eastern jetty.

Ten vessels have been reported aground in the pass and its neighbourhood, but in every case they were found to be out of the channel, which was amply wide and deep for navigation. At the head of the passes no work has been done since 1879; the mattress work is in most places below flood-level, and is buried in deposit; 720 feet of the piling of the dam remains, while 120 feet has been washed out: the foundation mattresses are in place throughout, with a depth of 23 feet of water over them; the mattress sills across south-west and north-east passes are still in place, but show a settlement of about 2 feet for a length of 1,600 feet.

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[1] The original is in the library of the Inst. C.E.

Respecting the condition of the original or main jetties, the east jetty, the concrete capping of which was damaged by the cyclone, is securely protected by sedimentary deposits on either side for a length of 5,700 feet, and the west jetty for a length of 4,800 feet. The concrete capping, where not disturbed by the storm, shows a subsidence during the year of 0·25 to 0·27 feet, and during the last three years of 1·12 to 1·28 foot.

Surveys below the jetties, extending out to a depth of 100 feet of water, show very little change to have occurred on the bar during the year, the 40-feet and 50-feet depth contours have advanced seaward; the change, however, taken as a whole, has been slight, but comparing the mean depth over the whole area with that in 1876, there appears to be a diminution in the proportion of 72 to 78.

The above surveys extending over a fan-shaped area, reaching from lower ends of the jetties to the 100-feet line, show the following mean depths throughout the whole area:—

1876.	1877.	1878.	1879.	1880.	1881.	1882.	1883.
60·78	60·38	62·18	59·62	59·74	57·36	56·06	56·10

A. T. A.

The Nira Canal.[1]

By J. E. Whiting, M.A., M. Inst. C.E.

This canal is designed for the irrigation of the Nira and Bhima valleys, for a household supply to the towns and villages where requisite, and also for water-power. The construction of the canal was undertaken, in the first instance, with the view of protecting from famine the district of East Poonah, which had been subject to failure of crops many years in succession, owing to the uncertainty of the rainfall. The construction was commenced in 1877 as a famine relief work. The number of persons employed rose rapidly from 5,000 to 24,132. At the termination of the famine, towards the end of the year (1877), the works were closed, but were re-opened the following year. During twenty-six months the average number of persons of all ages employed was 8,096.

The canal lies on the left bank of the Nira river, and extends for a length of 103 miles, exclusive of distributing channels. It commands 280,000 acres of arable land, and will furnish an unfailing supply of water to 106,500 acres. The Nira and its branches rise in the Sahyadris, and have a catchment-area of

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[1] The original is in the library of the Inst. C.E.

700 square miles at the headworks of the canal. In the south-west monsoon, from the middle of June to the middle of October, the river discharges far more water than can be carried by the canal, and in ordinary seasons there is a considerable flow until the end of December; but to ensure a supply during the dry season extensive storage will be required. A reservoir 19 miles in length, with an area of 7¼ square miles, is to be formed on the Velvandi, one of the tributaries of the river, by a masonry dam 3,000 feet in length, and 100 feet in height. This reservoir will have a capacity of 4,641,000,000 cubic feet, which can be increased to 5,500,000,000 cubic feet by closing the shutters designed for the weir. The estimated cost of the storage work is 181 rupees per 1,000,000 cubic feet.

The headworks of the canal are at a point 19 miles farther down the river, near the junction of the Vir and the Nira, where a concrete weir faced with rubble masonry, 42 feet high, and 2,300 feet long, has been built across both rivers. This will raise the water to the full supply-level of the canal, to which it will be admitted by large iron sluice-gates. The basin thus formed above this weir will extend for 11 miles in length. As an additional work to the canal, two large reservoirs have been designed to be supplied from the canal during the south-west monsoon, as they have but little catchment-area. These will increase the storage available during dry weather for the end of the valley most distant from the main reservoir. In several places, owing to the hilly nature of the ground, the canal has been carried through cuttings ½ mile in length, and 35 feet in depth at the centre. Twenty aqueducts have been built over large water-courses, in addition to ninety-four culverts and nine over-passages. The largest aqueduct consists of thirteen spans of 30 feet, with 23 feet headway, and carries the canal over the Karha stream, which drains 440 square miles. The over-passages consist of large inverts in which the streams flow, in two cases 90 feet in width, while the canal passes underneath in two arched galleries. In most cases the aqueducts and culverts have been constructed so that carts and cattle can pass under them: these, with thirty-seven road- and accommodation-bridges, and several foot- and cattle-bridges, provide crossings at an average of about every ½ mile of the canal.

The first 52 miles of the canal is expected to be opened so as to utilize the Nira water in the monsoon of 1884.

A. T. A.

Irrigation in the Alps.

(Bulletin de la Société Vaudoise des Ingénieurs et des Architecte, 1883, p. 49.)

In the canton of Valais a system of irrigation-channels has been constructed in the mountains, which, in the course of years, has attained an extraordinary development and importance, the inhabitants being dependent upon it for their means of subsistence.

The works have been carried out in difficult, sometimes almost inaccessible positions, in regions subject to earthquakes and avalanches.

The canton suffers from severe heat and drought in summer, and this has been particularly the case of late years, since it has been to a great extent disforested, and, in consequence, rain has become more and more rare. As, however, the population increases, it becomes necessary to increase the area of land under cultivation, and this can only be done by providing water by means of irrigation-channels.

A few of these channels date back to the time of the Romans, some were constructed during the middle ages, but the greater number are much more recent. Some have their sources at elevations of 8000 feet, and extend to a length of 25 to 30 miles. Some idea may be formed of the difficulties encountered in their construction from the following particulars. In many places it has been necessary to build long lengths of retaining-walls on one side, and of revetment-walls on the other; tunnels a quarter of a mile long have been driven; for lengths of half a mile, ledges have been cut into vertical faces of rock. In places where the rocks overhang, the water is carried in wooden troughs, supported by iron bars having one end firmly fixed in the rock, the other suspended from a chain or tie-rod. In order to get them into position the workmen have to be let down by ropes, which in one instance had to be 4000 feet in length. For crossing ravines, arches up to 65 feet span have been built. Where vertical falls occur (one of these is 1000 feet high) the water is carried in a wooden box to prevent loss by dispersion. The channels are generally from 1 foot 6 inches to 2 feet 6 inches wide and deep; the gradients are most frequently between 1 and 1·5 per 1000, sometimes as much as 2·5 per 1000.

The works are designed, laid out, and constructed by the mountaineers without technical assistance. They use a rough kind of grading tool, made of a mason's level and a straight-edge, of which the lower edge is cut to the required gradient. Many miles of channel are graded with this instrument with a fair degree of accuracy, but as mistakes sometimes occur it is now becoming customary to have the lines levelled in the ordinary way.

In many of the valleys several tiers of channels are to be seen one above another, the reason being partly that each commune prefers to have its own separate channel, partly because the difficulty of making one large channel is greater than that of making several smaller ones. The properties of the waters differ considerably, some being much more valuable for irrigation purposes than others, and in some cases long channels are constructed for the purpose of obtaining water rich in fertilising ingredients, when an inferior quality could be obtained close at hand. The water is generally drawn from glaciers, and is therefore too cold for immediate use. To remedy this it is conveyed as far as practicable along the sunny sides of the valleys; and parts of the course are specially laid out for warming it in the sun. When gravel is brought down by the water, catch-pits are built at intervals, in which it is deposited, and which are cleaned out when necessary.

The channels require constant supervision, and an ingenious device is made use of for warning the inspector when the flow of water is abnormal. It consists of a small water-wheel working a hammer, which strikes upon a wooden plank. Any irregularity is detected by the inspector at a distance by a difference in the sound produced by this machine.

The total length of these channels in the canton is nearly 1000 miles. The water is used for irrigating pasture-land, but is even more valuable for vineyards, the yield from which is doubled by the process. The author estimates that a capital sum of £280,000 has been expended on the channels, and the annual cost of maintenance is considerable.

W. H. T.

Report of the Ministry of Public Works on the Irrigation of Egypt, and an Examination of a Proposal relating to it.

By Rousseau Pasha, Director-General of Public Works. Approved by Aly Pasha Moubarik, Minister of Public Works.

(Cairo, April 24, 1883.)

After alluding to the circumstance that the origin of the report must be referred to a recommendation made on the 24th of January, 1883, by the Ministry of Public Works, to the Council of Ministers to improve the irrigation of the country by means of steam pumps, and a proposal renewed about the same time by Messrs. Suarès and Consorts to undertake the irrigation of the whole of Lower Egypt, the report goes on to declare that the efforts of Government must, for the present, be confined to improving the irrigation of lands already under cultivation, and proceeds to discuss the systems in vogue.

In the first place the report treats of the methods of irrigation extant, of their development and maintenance, and shows that in Upper Egypt, from Assouan to Cairo, the ancient system of inundation by sections must be, in the main, adhered to, because this system permits of a certain amount of regulation of the flood waters, and makes no demand on the summer flow. Along the river banks, and also along those of the Ramadi and Ibrahimieh canals, a small proportion of summer cultivation is however carried on by the aid, for the most part, of local pumping stations. Cultivation by inundation is easy, the land is greatly improved by warping, and the works of maintenance are unimportant, demanding little recourse to forced labour. To improve this system a comparatively small expenditure is required in regulating the dykes and canals, and in constructing permanent sluices where now the barbarous system of annually cutting through and making good the banks is practised. The question of extending the summer cultivation to Upper Egypt is discussed, and shown to be undesirable, on the ground that such an extension would seriously reduce the already scanty summer waters to which Lower Egypt has a claim, arising from the fact that the whole of the Delta has for the last century been cultivated by irrigation.

The grand schemes, some of ancient date, of constructing regulating basins in higher reaches of the river are mentioned, but set on one side as beyond the scope of the report.

The requirements of Lower Egypt are next discussed, and the quantity of water necessary calculated on the supposition "that the sequence of the crops will be determined, not as it is at present by the exigencies of a niggardly irrigation, but as it will be when the Government, by an act of elementary justice, will not only extend over the whole land the summer irrigation-canals, but will also provide the water necessary to keep them full."

The whole of Lower Egypt is traversed by a treble set of canals. Those called Nili, shallow and wide, intended to convey the water of high Nile; secondly, those called Sefi, deep enough to take the waters of low Nile; and, thirdly, the drainage-canals which are intended to carry off the waters after they have done their work on the land. The Sefi canals have their origin, almost invariably, in the Nile; the Nili canals are mostly supplied by the Sefi where the river rises high enough.

The object of the Government, for the moment, is to convert the Nili into Sefi canals, so as to enable the farmers of the Delta to raise every kind of crop; but the difficulty of obtaining the necessary labour has proved insurmountable, because whether it be forced or free the number of hands required is so enormous that if employed the agricultural operations in the field would come to an end.

The use of mechanical means of cleansing and deepening canals has been well considered, but even if practicable would require much time to perfect, "and the country cannot wait."

The report goes on to consider the proposition of raising the waters of irrigation by artificial means, and points out the following advantages: A great saving of forced labour in its most odious form, which consists in compelling the people to work at a distance from their homes; the power of commanding a fixed level in the irrigation-canals independently of the level of the Nile; the considerable saving which will be effected for several years in the cost of cleaning out the Sefi canals, because these may be allowed to silt up very considerably; the abolition of private water-raising appliances, which are most extravagant in their operation; and, lastly, with hardly any expenditure, converting all the shallow Nili canals into watercourses for summer cultivation. A reservation is, however, made with respect to certain Sefi canals which act as auxiliaries in carrying the flood waters of the Nile to the sea.

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The drainage-canals are then dwelt upon. Their importance is recognized, and the neglect into which they have fallen deplored.

The possibility of making the Barrage serve the purpose for which it was intended by Mohamed Aly is next discussed. The intention was, at low Nile, to raise its waters above the Barrage about 14 feet, and by means of three arterial canals to convey the water through the Delta at a level to command the land at all times. The objections urged against this project, supposing even that the Barrage had been rendered capable of withstanding the pressure it was intended to bear, are: The danger of altering the regimen of such a river as the Nile by the immense accumulation of mud which is sure to take place above the weir, the great cost of completing the arterial canals and of maintaining them afterwards, the danger of infiltration from the water level being too high in places, and the imprudence of placing two million acres of land at the mercy of a single structure. The Barrage, it is pointed out, now serves an invaluable purpose, quite different from that originally intended, namely, that of regulating the relative quantity of water flowing, at Low Nile, down the two branches, the tendency being for the water to follow the shorter or Rosetta branch.

Next follows a tabular statement of the areas to be irrigated in the Delta, and a discussion as to the quantity of water required. The calculations are based on the fact that one-third of the total area only is, on the average, under summer crops requiring irrigation, and that 22 cubic metres per acre per day is a sufficient supply for that area. Certain additions are made for special cases such as navigation, the supply of Alexandria, the extension of cultivation, and the grand total arrived at of 2,803,000 acres, requiring a supply of 25 million cubic metres of water per day, or nearly 9 cubic metres per acre per day, over the total area of Lower Egypt, for the summer crops.

The discharge of the Nile at low water is taken at 38 million cubic metres per day, and the 13 million cubic metres which remain after the wants of irrigation have been satisfied, are to be divided equally between the Rosetta and Damietta branches.

The report goes on to deal with the question of forced labour. It recognises the fact that this method of accomplishing the immense amount of earthwork which has to be done in a very short time, is becoming more and more repugnant to the spirit of the age, but at the same time declares that the day is yet far distant when forced labour can be completely dispensed with. In Upper Egypt, from the nature of the cultivation, the system presses less severely than in the Delta. The total number of men engaged at one time has recently been 160,000 divided about equally between the two divisions of the country. Forced labour is still levied on a census made 36 years ago, and represents very imperfectly the present state of things. The means of reducing the pressure on the people is fully discussed, and it is shown that by certain changes in the laws important improvements may be effected.

The concluding portion of the report is occupied by estimates of the cost of carrying out the recommendations made, and the financial arrangements by which the expenditure might be met. The cost of the works required for Upper Egypt, to be executed in from 10 to 15 years, is put down at £1,376,000, making an annual charge for interest and sinking fund maintenance and supervision of from £92,200 to £137,000. For Lower Egypt ten large pumping stations are proposed in addition to the two already existing; the total quantity of water to be raised is set down at 19,300,000 cubic metres per day, the lifts varying from 8 feet to 16 feet, and the time of running ranging from 120 days to 240 days in each season. The capital expenditure is set down at £1,775,000, and the annual charge, calculated, as in the case of Upper Egypt, is estimated at £624,000.

This expenditure is to be met partly by a tax on the land which would amount to a little over 2*s*. per acre, partly by a poll tax levied on those liable to forced labour, but whom the contemplated works would free from such service, and partly by contributions from the Imperial revenues. With respect to the capital sums required it is proposed in many cases to create concessions, terminable in thirty-five years, with private individuals or companies.

To carry out the recommendations of the report certain changes in the existing laws will be necessary, and these alterations are pointed out in detail.

W. A.

New Docks at Antwerp. By — DE KEYSER.

(Bulletin de l'Association des Ingénieurs sortis de l'École de Liége, new series, vol. viii., 1884, p. 19.)

The municipality of Antwerp purchased from the State, in 1881, the site of the North Citadel, having an area of 84 acres, for the sum of £151,000, with the object of extending their dock accommodation to the north of the existing docks. A further area of 25 acres is being acquired to connect the two dock-estates together. Mr. Royer's design for the new docks, now in course of construction, was approved in 1883. The present water-area of the city docks is about 106 acres,[1] and the extension will raise the area to almost 160 acres, exclusive of the river-craft docks of 15 acres. The works comprise two large docks, having a joint area of about 50 acres, with a length of over 11,000 feet of quays, which is equivalent to the new river-quays constructed by the State. The Africa dock, destined for transatlantic steamers, is to be excavated to 18 feet below low water, and will afford an average depth of water of 30 feet; it is 1,955 feet long and 607 feet wide. It will be joined to the old docks by a channel

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[1] Minutes of Proceedings Inst. C.E., vol. liii., p. 313.

59 feet wide, leading into a basin 377 feet long and 164 feet wide, from whence access will be obtained to the Kattendyk dock through a lock 59 feet in width, having a depth of 22½ feet on the sill. The Africa dock is to be eventually given a new entrance into the river to the west, through a lock having lower sills than the entrances to the present docks. The second new dock, called the America dock, will communicate with the Africa dock through a passage 279 feet long, having a minimum width of 59 feet, and a depth of 30 feet below the ordinary water-level of the docks. The America dock, destined for the petroleum trade, has a length of 1,394 feet, and a width of 607 feet. Like the Africa dock, it will be surrounded with quay-walls, similar in construction to those of the Kattendyk dock, except on the west side, where it will be terminated by a pitched slope of 1½ to 1, with a view to further extensions. The estimated cost of these works was £300,000, and they were let by contract, in 1883, for £231,400. The works are in active progress, and are to be completed in the middle of 1886.

L. V. H.

Hygienic Exhibition at Berlin, 1883. *Report on Group* 21, *Water-Supply.* By G. Oesten.

(Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege, vol. xvi., p. 69.)

The various schemes are dealt with by the Author first under the head of "Preliminary Technical Investigations and Projects for Water-works." Among these are included the researches of Thiem for the supply of water from the Rhine valley for the town of Strasburg in 1874–75, as also those for Augsburg and Munich. Similar plans and details relating to other towns were contributed by Gruner and Dr. Hulwa. In connection with the scheme of the latter engineer for the drainage of Breslau was a diagram showing, by means of various tints, the degrees of impurity in the water of the Oder, above and below the town, as ascertained from numerous analyses. The facts adduced would appear to make out a marvellous power of oxidation for the water of that river. Taking the figures given for the proportion of ammonia, there were in each 100,000 parts:—

	Parts per 100,000.
1. Before any of the sewers enter the river	20·80
2. Within the town at the outfall of the sewers . . .	1034·00
3. Just below the town, or from 1 to 2 kilometres further	112·40
4. 9 kilometres below the town of Breslau	48·00
5. 14 „ „ „	17·50
6. 32 „ „ „	15·40

Arguing from this Table, the Author observes that the river would appear to manifest the greatest self-cleansing powers where it is most foul, namely, within the town itself, and for 1 or 2 miles below it. He points out, however, that the above figures are to some extent fallacious, because the volume of the Oder is given as equal on an average to 148 times that of the sewage; and if the 20·80 parts of ammonia extant in the river-water before admixture with the sewage be deducted from the 1,034·00 parts representing the mixed liquid, it would appear that, to bring about such a result as that above indicated, the sewage must contain 149,953·6 parts of ammonia in each 100,000 parts, which is manifestly impossible. The figures given by Dr. Hulwa must, therefore, be taken to represent sewer-water, and not a mixed liquid; and the apparent rapid improvement claimed for the water nearest the town would thus be due to simple diffusion of the sewage through the entire volume of the Oder water.

Schemes were also shown by Salbach for the supply of a large district in Silesia, and a report on the waterworks of Trieste, was submitted by a special commission charged to inquire into the subject.

The second division of the report relates to "Executed Schemes of Town Water-Supply." The waterworks of Aachen were illustrated by sixty sheets of careful drawings, dealing with every aspect of the subject. The most noticeable feature of this undertaking is the nature of the collecting-area. A drift has been pierced through the mountain, with an average section of from 2 to 4 square metres (21·53 to 43·05 square feet), and a total length of 2,400 metres (2,624·72 yards), the average depth below the surface being 35 metres (114·83 feet). It would appear that the formations traversed were mainly carboniferous limestone and shale. At some distance from its mouth, the drift is entirely closed by a dam, through which the mains only pass. Outside the dam is the compensation-reservoir. The drift serves at the same time as a storage-reservoir, and will permit of an accumulation equivalent to a head of 30 metres (98·42 feet) above the sill of the dam. The annual range of temperature of the water is only between 9·2° and 10° Centigrade (48·5° and 50° Fahrenheit). The minimum temperature is reached in May, and the maximum during the winter months. The total cost has amounted to 1,918,067·77 marks (£95,903) for a population of 80,000.

Details are given of the works for Barmen, from the designs of Schulke, at a cost of £112,500; also those of Elberfeld, Dortmund, Erfurt, and many smaller towns. The Hamburg waterworks were copiously illustrated, and among the models was one showing the ordinary arrangement for domestic water-supply and house-drainage, the former consisting of a cistern provided with a ball-cock, placed in a somewhat inaccessible position in the ceiling of the dining-room. Notwithstanding the fact that only unfiltered Elbe water is supplied, the consumption has risen, from 25,000 cubic metres per diem in 1858, to 105,000 cubic metres (about 23,000,000 gallons) per diem in 1882. Comprehensive plans were exhibited of the water-supply of Carlsruhe, and all that has been done by the Government of the Grand Duchy of Baden with respect to the supply of water to eighty communities, with an aggregate population of 42,000 inhabitants. It would appear that the direction and control of the water-supply has been undertaken by the State since 1878, and that at the present time twelve schemes are in progress of execution, and seventy plans are under consideration by the Government engineers.

Particulars are given respecting the works for Magdeburg, Berlin, Munich, Vienna, Chemnitz, Wiesbaden, and Frankfort-on-the-Maine. Stuttgart is distinguished as having two entirely distinct services, the one being spring-water for drinking purposes and the other being filtered water from the Neckar. The Author concludes with a brief account of the filters, water-meters, fittings, hydrants, &c., many of which were shown in operation at the Exhibition.

G. R. R.

Berlin Hygienic Exhibition.—Report on the Disposal of Sewage-Water, Excreta, and Waste Matters. By — MILCZEWSKI.

(Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege, vol. xvii., 1884, p. 82.)

Utter unconcern in sewage-matters prevailed in Germany until 1860; the causes for this indifference are briefly reviewed. Since then a controversy has raged on the subject of sewage treatment, one of the fundamental errors in regard to which has been the attempt to bring about a separation between the solids and liquids, which can never be satisfactorily accomplished. Of course these observations do not apply to the removal of road scrapings and ashes, which presents few problems, as these substances merely require rapid and systematic transportation from the vicinity of dwellings. Whether they can be dealt with at a profit is another consideration. The disposal of polluted waters on the other hand, that is to say of those which contain, in a naturally fluid form, fecal matters, urine, kitchen and domestic slops, bath and house drainage, the waste water of manufactories and industrial operations, and the rain-water from street surfaces and roofs, has been the subject of much careful consideration, and this problem can only be satisfactorily solved if systematic sewering is recognised as the sole reliable remedy. But sewers entail the existence of a water-supply, and in the absence of water no community can satisfactorily free itself from impurity and filth. All systems under which the excreta are stored in dwellings, in containers of every description, are objectionable. Where this plan is the only one possible, it may be laid down as an axiom that the smaller the vessels, and the more often they are emptied or removed, the better will it be for health.

The Author, having stated the foregoing facts, reviews the various schemes wherein water-carriage is avoided, shown at the Berlin Hygienic Exhibition, under the following heads:—

a. Those which aim at rendering the excreta free from nuisance at the points where they are produced.

b. Those intended to accomplish the removal of the fecal matters, free from smell, and to convert them into valuable manures.

The next division comprises the various schemes for the sewering of towns, and the removal of foul matters by the agency of water. The importance of this class of exhibits proves the interest it has evoked, and how general is the effort to introduce improvements in the method of sewage disposal.

Unfortunately but few German towns can boast of a complete system of sewering. Among them Bunzlau, Hamburg, Altona, Frankfort-on-the-Maine, Dantzic, Breslau, and Berlin are named. The reasons for the slow progress in this direction which has been made in Germany are considered, and the Author proceeds to examine the various schemes shown at the Exhibition. All the above-named towns, with the exception of Altona, sent plans. The facts in regard to Bunzlau are of great interest. The town architect, Doerich, sends the copy of a plan dated 1773, which sets forth that the sewers were originally constructed in 1531, and that the existing irrigation area of 15 hectares (37·06 acres) was laid out in 1559; that, up to 1866, 2,892 lineal metres of sewers had been completed, since which date an additional length of 826 metres of sewers, and 1,436 metres of pipe-drains have been laid; and that 48 per cent. of the area of the town is sewered. This drainage scheme, combined as it is with the use of the foul water for irrigation, proves that the idea of the disposal of town sewage by this means did not originate in England, but in a German town. Particulars are given of the dimensions of the sewers, and of the order in which the water is distributed to the different plots; these are laid down as permanent meadows. The grass is cut four or five times annually, and yields from 160 to 180 centners of hay per hectare (equal to 65·4 cwts. to 73·6 cwts. per acre). Notwithstanding the continued use of these meadows for so long a period (over 300 years), there has been no clogging or saturation, nor has there been any complaint of unwholesome exhalations on the part of persons living in the vicinity.

The oldest German drainage-scheme completed during the present century, is that of Hamburg. After the fire in 1842, the newly-erected quarters of the town were provided with sewers which were completed in 1848, and subsequently the older portions of the town have been successively sewered. The sewers are constructed of brickwork, and the larger ones are navigable by boats. Ventilators and man-holes are introduced at frequent intervals. To prevent the entry of river-water at high tides, the outfalls are provided with self-acting flood-gates. During long continued floods, the sewers act as storage reservoirs, but storm outlets into the Alster have been provided. There are four separate sewer systems in Hamburg, and the neighbouring town of Altona was sewered on a similar plan between the years 1857–75. Very complete plans and models of all that relates to drainage and water removal in Hamburg were shown.

The sewerage of Frankfort, Dantzic, and Breslau, was also shown by means of plans and models.

The sewer-system of Berlin, which is the most recent work of the kind undertaken in Germany, was amply illustrated by drawings. The total area of land under irrigation on the various sewage-farms at the present time is 5,374 hectares (13,279 acres), and this is very nearly equal to the area of Berlin to be eventually drained, which is given at 5,769 hectares (14,255 acres). The arrangement of one of the irrigation-areas, that of Osdorf, 1,242 hectares (3,069 acres) in extent, was shown in detail. It receives the sewerage of three of the radial systems of Berlin by means of two cast-iron rising mains, respectively 1·00 metre and 0·75 metre in diameter. These mains branch into a number of smaller ones on reaching Osdorf, and distribute the water over the farm, which is laid out in plots of an average area of 2½ hectares (6·17 acres) each, as nearly as possible rectangular in form. The plots are surrounded by footpaths. Those fields which are fairly level are laid out in horizontal beds; those which are on a slope are utilized as meadows, with a fall in one direction. In addition to these smaller areas, several larger horizontal plots have been surrounded with raised banks, 1 metre in height, on which are roadways. These enclosures serve as storage basins for the water in autumn and winter, and in the proper seasons they are sown with corn and root-crops. In addition to drawings, full-sized models were shown of house connections, street gullies, &c.

Among numerous towns, the drainage of which was exhibited by plans and drawings, the Author enumerates Augsburg, Pesth, Chemnitz, Düsseldorf, Carlsruhe, Stettin, Szegedin, and Vienna.

G. R. R.

Mineral Oil as Fuel in Steam-Boilers. By — DE MAUPEOU.

(Note sur l'Emploi des Huiles minérales pour le Chauffage des Chaudières, September 1882.)

Mr. St. Claire-Deville analysed a great number of samples of petroleum, and after having tested Audouin's system in his laboratory, he applied it, in co-operation with Mr. Dupuy de Lôme, on the 'Puebla'; and also with Messrs. Sauvage and Dieudonné in 1868 on two locomotives on the Eastern Railway of France.[1] The apparatus consists essentially of a number of small jets, from which the oil is distributed in as many canals slightly inclined. These are succeeded by cast-iron grate-bars, grooved, nearly upright, with air-spaces between them. The oil commences to distil in the canals, and it burns on the bars, which attain a high temperature, and act like long matches. The consumption of oil was remarkably uniform. The results of lengthened trials with engine No. 291 proved that the oil could be burned with absolute smokelessness; and the respective consumptions were, of oil, 21 lbs. per mile run, and of coke, 32 lbs. per mile run, showing that the same work was done with a consumption of oil two-thirds in weight of the coke consumed. This relation is in perfect accordance with what was obtained on the Puebla. With No. 192 engine, fitted with the Ten-Brink furnace, the respective weights of water evaporated per pound of fuel were, for oil, 10·9 lbs., and for briquettes of good quality, 7·9 lbs. In this case, 38 per cent. more water was evaporated by the oil. The apparatus worked very well.

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[1] The results of the trials on the railway have been published in the Comptes-rendus of the Academy of Sciences, vols. lxviii. and lxix.

The Compagnie des Forges et Chantiers, Marseilles, made trials of heavy oils in combustion for generating steam on Cody's system, on a cylindrical boiler with an inside furnace, and on another having tubular flues, in both cases with forced draught. The results were sensibly the same with respect to the combustion of the oil, without any discharge of smoke. The heavy oils were such as are obtained in gasworks as residuary products, having the density 1·04. They do not take fire except at high temperatures; they emit very little odour and very little vapour.

The apparatus was similar to that employed on the Eastern Railway, with an improved mode of regulating the supply of air between the bars of the grates. The tests to be noticed were made with the ordinary type of marine boiler—cylindrical—having a fire-tube, with return flue-tubes. The oil is supplied to the furnace from a tank placed above the level of the grid, in which a steam-pipe is laid at the bottom to heat the oil and keep it very fluid. The oil is filtered through a strainer to keep back solid impurities. The apparatus consists essentially of three parts—the distributor, the feed-tubes, and the burner. The distributor serves to regulate the supply of oil to the feed-tubes, by means of two tubes, one within the other, pierced with corresponding holes, the passage through which may be opened or closed by a movement of the inner tube. The feed-tubes conduct the oil from the distributor to the burner; they are of sufficient length to enable the liquid-column to balance the wind-pressure in the furnace, and, when empty, they are closed by a valve at the lower end of each, by which the escape of air is prevented. The burner is formed of cast-iron bars, lightly grooved or guttered, on which the oil is discharged from the feeding tubes, and burns as it runs down, the remainder falling into the "ash pit," a small trough at the lower end of the bars, open to the furnace. Between the bars, air-distributors of cast-iron are placed. These are hollow, and made with perforations, through which air is delivered for combustion with the oil. A fire-brick bridge is constructed, facing the burners, and sloping backwards, to give a suitable direction to the flame, and to radiate the heat sufficiently to distil the oil as it flows.

The shell of the boiler is about 80 inches in diameter and 9 feet long, with a single fire-tube about 37 inches in diameter, and return flue-tubes 2¾ inches and 3 inches in diameter, and 5½ feet long. The chimney is 23 inches in diameter, and 19 feet high. In the apparatus there are ten gutter-bars, 9½ inches long, of which the combined length is 95 inches, or say, 8 feet. The bars are about 2 inches apart between centres. The heating surface is 355 square feet. There is here a much larger proportion of gutter-burner than in the locomotives on the Eastern Railway.

The leading results of trials of this boiler worked by compressed air at 8 centimetres and 12 centimetres, or 0·315 inch and 0·472 inch respectively, are as follows:—

Pressure of air-supply	0·315 inch.	0·472 inch.
Smoke	None.	None.
Oil consumed per hour	158½ lbs.	286 lbs.
” ” per lineal foot of gutter-burner (say)	20 ”	36 ”
Temperature of the feed-water . .	102° Fahr.	102° Fahr.
Water evaporated per hour . . .	..	..
” ” lb. of oil . .	13·2 lbs.	13 lbs.
Burnt gases: composition CO^2 . .	12·5	12·0
Burnt gases: composition O . . .	4·5	4·5
Burnt gases: composition CO . .	1·5	2·0
” temperature	480° Fahr.	580° Fahr.

In the first case, the oil appeared to be entirely consumed on the gutters; in the second case, part of the oil was consumed in the base-trough, with a light deposit of coke. The boiler performed its usual duty without any difficulty.

Special tests were made in presence of the Author with regard to the employment of the system in torpedo-boats. To light the furnace, a few chips and shavings are burned under the grate, so as in the course of eight or ten minutes to heat it sufficiently to inflame the oil. The fan is started, and the supply of oil is gradually increased. In from twenty-five to thirty minutes after lighting, the pressure begins to rise; and twenty to twenty-five minutes later it attains to 5 atmospheres. From 160 to 180 lbs. of oil have been consumed in getting up steam. The supply of air should be just sufficient to prevent smoke; an excess, of course, leads to a reduction of efficiency. After many trials, the last trial was made in August 1882, under quite satisfactory conditions, with the following results. The trial lasted two hours twelve minutes; the oil was consumed at the rate of 197 lbs. per hour, or 25 lbs. per foot of gutter-bar; and 13·3 lbs. of water were evaporated per pound of oil. The blast-pressure was from 8 to 9 centimetres, or about 0·34 inch. Under like conditions, 8½ lbs. of water are evaporated per pound of good coal, and the equivalent quantity of coal required to evaporate an equal quantity of water would be 308 lbs. per hour, or 22 lbs. per square foot of grate per hour. The oil costs £8 1*s.* per ton against 20*s.* per ton of coal; so that the oil costs five times the cost for coal for equal duties. After the trials, it was found on inspection that the bridge was partly vitrified by intense heat, and deposits of coke were formed on the face of the bridge and in the base-trough. The boiler and the grate were in good condition.

Mr. Maupeou concludes that the system of burning heavy oil in steam boilers can be advantageously employed in torpedo-boats. It is easily managed, easily adapted to requirements, and involves a less weight of fuel than for coal-burning.

D. K. C.

Comparative Experimental Analyses of Steam-Engines.

By O. Hallauer.

(Bulletin de la Société Industrielle de Mulhouse, April–June, 1884, p. 255.)

This paper was the last communication of the late Mr. Hallauer, the results of whose frequent and important investigations of the action of steam in the cylinder have from time to time been noticed in the Minutes of Proceedings. It was written, by the Author, for the *Société des Ingénieurs-civils*, Paris, and it has been published simultaneously in their minutes, and in the Bulletin above noted.

Mr. Hallauer reverts to one of his earlier studies,[1] as a text for a brief communication to the *Société*. In that paper, he showed that two steam-engines under analogous conditions, on their maximum performances, consumed experimentally the same quantity of steam per horse-power, irrespective of their dimensions, that is to say, of the quantities of power which they were capable of developing, which were, in the case considered, 85 HP. and 8,500 HP., yielded by a Corliss engine and a marine engine respectively. The engines were each of one cylinder, having each four valves for cutting-off and exhausting. The data observed and deduced are ranged in a Table given with the paper.

Comparing first, the consumption per I. HP. per hour, the steam-jacket appears to be of greater benefit when the expansion is more prolonged. Thus an admission of 6 per cent. shows an economy of $\left(\frac{26 \cdot 98 \text{ lbs.} - 18 \cdot 57 \text{ lbs.}}{26 \cdot 98 \text{ lbs.}}\right) = 31$ per cent.; whilst, for an admission of 14 per cent., the economy was only $\left(\frac{24 \cdot 25 \text{ lbs.} - 18 \cdot 18 \text{ lbs.}}{24 \cdot 25 \text{ lbs.}}\right) = 25$ per cent. The apparent efficiency is, in part, dependent upon the back pressure of exhaust; and, omitting this element for a moment, and calculating for the absolute positive pressure, the respective economies would be as follows:—for 6 per cent. admission,

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[1] "Machines Fixes, Machines Marines," in the Bulletin, 1880.

$\frac{22\cdot57\text{ lbs.} - 16\cdot64\text{ lbs.}}{22\cdot57\text{ lbs.}} = 26\cdot2$ per cent.; and for 14 per cent. admission, $\frac{21\cdot52\text{ lbs.} - 16\cdot51\text{ lbs.}}{21\cdot52\text{ lbs.}} = 23\cdot3$ per cent.: showing a comparatively small difference of 3 per cent. in the economies of the envelope. But this does not prove an identity of action in the two cases. The jacket diminishes in a special proportion the quantity of condensed steam at the end of each admission; thus, taking the differences of the percentages of condensed steam, without and with steam in the jacket, that is, the diminutions of initial condensations by the use of the jacket:—

56·4 per cent. − 46·5 per cent. = 9·9 per cent. for 6 per cent. admission.
48·7 ,, − 29·5 ,, = 19·2 ,, ,, 14 ,, ,,

Whilst, at the end of the expansion, the differences or diminutions of condensation by the use of the jacket are more evenly alike, thus:—

38·8 per cent. − 20·1 per cent. = 18·7 per cent. for 6 per cent. admission.
37·6 ,, − 15·4 ,, = 22·2 ,, ,, 14 ,, ,,

Giving rise to a difference of relative re-evaporation, greater in the case of the more prolonged expansion, thus:—

18·7 per cent. − 9·9 per cent. = 8·8 per cent. for 6 per cent. admission.
22·2 ,, − 19·2 ,, = 3·0 ,, ,, 14 ,, ,,

Finally in this connection, the quantities of heat taken from the cylinder, by evaporation during exhaust, into the condenser are correspondingly:—

29·0 per cent. − 12·6 ,, = 16·4 per cent. for 6 per cent. admission.
25·2 ,, − 7·0 ,, = 18·2 ,, ,, 14 ,, ,,

The small difference of these final percentages follows from the small difference, above noted, of those for the end of the expansion.

Mr. Hallauer directs attention to the paradoxical fact that the use of the jacket should, by a condensation of steam not exceeding 6 per cent. of the total weight of steam, augment the horse-power from 82 HP. to 102 HP. for 6 per cent. admission; and from 132 HP. to 162 HP. for 14 per cent. admission. The effect is due partly to the reduction of initial condensation in the cylinder during admission, and partly to augmented re-evaporation during expansion: in proportion, varying with the period of admission.

The results of performance of the marine engine are next considered in the same manner, and compared with those of the Corliss engine. Loyal to his chief, Mr. Hirn, to the last, the Author gratefully ascribes to him the discovery that the merit of the steam-jacket of Watt is, above all, to communicate heat at the right moment.

D. K. C.

The Schultz-Roeber Furnace. By H. WALTHER-MEUNIER.

(Bulletin de la Société Industrielle de Mulhouse, April–June, 1884, p. 233.)

In his report as engineer-in-chief to the Alsatian Association of Steam-Users for the year 1883, Mr. Walther-Meunier reverts to the Schultz-Roeber furnace or mechanical furnace, which has already been noticed.[1] A new series of trials has been made by the Author on a battery of four boilers, of which the furnaces had been reconstructed according to the latest modification, as already described. The results of these trials confirm those already communicated, and the Author reports the general results of two years' experience of the boilers.

With respect to smoke-prevention, the Schultz-Roeber furnace is inferior to the Ten-Brink furnace. It makes smoke at starting and resuming work after stoppages, and whenever the stoker neglects sufficiently to distribute the fuel on the grate. It has no advantage on the ordinary furnace in respect of management, whilst the cost for maintenance is greater, the fire-brick work wearing out quickly, and of course demanding frequent repair. The rate of production of steam is, as in gazogenes and like apparatus, not sufficiently elastic to suit varying demands for steam. Steam is produced during stoppages, and at night, resulting in waste of steam and damage by humidity to polished surfaces. After the dampers are closed, the partially-burned coal discharges smoke, which escapes by the door and the hopper, and leads to deposits of soot about the boiler-house. Though the evaporative efficiency is a little greater, on an average, than by ordinary stoking, yet it has occasionally been found to be lower. The cost of supplying and erecting the apparatus is from £60 to £70 per furnace. The Schultz-Roeber furnace may answer for continuous work and a uniform demand for steam.

D. K. C.

Cast-Iron Heads to Steam-Boilers. By S. LLOYD WIEGAND.

(Journal of the Franklin Institute, May, 1884, p. 348.)

A drum or cylinder, 36 inches in diameter and 4 feet long, was closed with flat cast-iron heads, and was tested by hydrostatic pressure till it was burst. The heads had a tensile strength of 18,000 lbs. per square inch; they were 1$\frac{5}{16}$ inch in thickness, and were riveted to the cylindrical portion. A manhole-opening about 10 inches by 14 inches, was cast in one of the heads, and below it a feed-inlet 2 inches in diameter. The manhole was closed by a cast-iron plate and a gasket of vulcanised india-rubber between the plate and the slightly raised border of inner surface of the end plate, which had been planed flat.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxiv., p. 345.

The test was made with a $\frac{5}{8}$-inch force-pump, and two pressure-gauges. At a pressure of 425 lbs. per square inch, the longitudinal seam leaked just sufficiently to show a wet line; so also did a small part of the seam of the cast-iron head having the manhole. At the pressure 550 lb. per square inch, the head cracked at two places radially from the manhole, showing very little leakage; it was also cracked for a length of about ten inches at the rim or flange where it joined the fillet, uniting it with the flat part of the head. At a pressure of 820 lbs. per square inch, a piece of the head, about one-fifth of the whole, fell out, and the vessel was emptied in three seconds. Up to the time of the piece breaking out, not more than half a pint of water had escaped.

The back head of the drum expanded very nearly $\frac{3}{16}$-inch in the experiment. It resumed its normal shape, and appeared to be unimpaired in strength.

D. K. C.

Explosions in Pump-Plungers.

(American Machinist, May 17 and 31, 1884.)

The issues of the "American Machinist" for the above dates contain some correspondence on pump-plunger explosions; these seem to have occurred in an almost precisely similar manner in several instances. In taking the pumps apart for repairs, the plungers were found to be in some cases full of water under pressure; in others the plungers appeared to be empty, but on a light being applied to examine the inside, a serious explosion was the result, and a quantity of peculiarly offensive blue mud was blown out.

It is supposed that the explosions were caused by an accumulation of hydrogen gas, generated by the action of the water which leaked into the plunger. This water uniting with the iron formed oxide, the oxygen of the water uniting with the iron and setting hydrogen free. The blue mud was probably an imperfect oxide of iron, containing carbon from the cast iron, which gave the offensive smell. Five years would suffice to produce the action.

W. P.

Sewage-Pumping Engines at Boston.

(American Machinist, May 31, 1884.)

The engines described were designed by E. D. Leavitt, Junr., and are erected at the pumping station of the Boston sewage-works.

The engines are a pair of compound engines of the beam and fly-wheel type, and were designed to meet special conditions of space. The high- and low-pressure cylinders are inverted and placed on opposite sides of the fly-wheel shafts directly over the pumps, and are coupled by short beams.

The high-pressure cylinders are 25½ inches diameter, and the low-pressure cylinders 52 inches diameter, the stroke of both being 9 feet. The fly-wheels are 36 feet diameter, and weigh 36 tons each; they are placed between the cylinders over the beam centres.

The steam is distributed by gridiron slide-valves driven by cams; the cut-off is regulated by the governor.

The sewage-pumps have single-acting plungers, 48 inches diameter by 9-feet stroke. The valve-chambers are polygonal frustums of cones, and surround the plungers, and each face carries a series of seats for rubber valves.

There are two locomotive-boilers for each pair of engines; each boiler is 31 feet 10 inches, by 6 feet 8 inches diameter. The fire-boxes are double, and open into a common combustion chamber; the tubes are 3 inches diameter and 15 feet long, and are one hundred and thirty-two in number. The boilers are of steel, $\frac{7}{16}$ inch thick in the shell. Economisers are placed in the flues.

The Paper is illustrated with drawings of both engines and boilers.

W. P.

Riedler's Indicator-Diagrams of the Action of Pumps.

By Louis Canon.

(Annuaire de l'Association des Ingénieurs sortis de l'École de Liége, January and February, 1884, p. 1.)

This article is a *résumé* of Mr. A. Riedler's work on "Indicator-diagram Experiments on Pumps and Draining-Engines." First, of the resistance of valves to open or lift, under the pressure of the super-incumbent water. Not with any kind of valve, or with any type of pump, has the fact of the resistance of valves been certainly established by means of the indicator, and yet it has generally been assumed that there is such resistance. The experimental researches of Mr. Riedler upon hundreds of pumps of various types, with valves of very various construction, and at various speeds, lead to the conclusion that no such resistance exists, except in a comparatively feeble degree—very much less than the enormous theoretical estimates of resistance. Besides, with the same pump, the seats have been changed, and those of excessive dimensions have been substituted, without affecting the diagrams in the smallest degree.

In a treble-ring seated valve, in a direct-action pump, contrary to ordinary calculation, the uppermost ring lifted first, then the next lower, and the third, or lowest and largest, did not lift at all.

The weight of valves materially influences their closing. The heavier they are, the less is their lift, and the more certainly and quickly they close. A light valve, being subject to vibration and influence of eddies when open, causes undulations of the pressure-line of the diagram. The character of the oscillations is very much influenced by the form of valves. Valves of sufficient weight never cause oscillations of pressure; the diagrams are regular and rectilineal. Loaded leather valves, hinged on themselves, always give rectilineal diagrams. But if hung on special hinges they loose the spring of the valve hinged on its own material, and give rise to oscillations of pressure. The lighter the valves and the greater the speed, the greater, also, are the oscillations of pressure. At the Amelung pits at Königshütte. double-beat valves of very light construction yielded diagrams of which the up-flow line was subject to violent fluctuations for the whole of the stroke. When replaced by weighted leather valves the oscillations disappeared.

The degree of pressure of the column of water affects the diagram, inasmuch as a diagram taken from a feed-pump under low pressure, with a light valve, may exhibit oscillations, whilst at high pressure the oscillations disappear. At the Kladno steel works the accumulator-pump with ordinary conical valves did not occasion any oscillations under the maximum charge, 30 atmospheres; but oscillations appeared when the pressure of the accumulator fell, in consequence of several hydraulic apparatus working simultaneously.

The indicator-diagrams prove that the suction-valves are of the greatest importance, since it depends on them that the pump should work without shocks. The required weight of the valves is to be calculated for particular cases. If the valve be heavy, it is to be maintained by the pressure of the water, and by so much the possible maximum height of the lift is reduced and the hydraulic resistances augmented. But very many valves commence to close when the velocity of the piston is slackened, and they unclose just before the change of stroke. Shocks arising from tardy closing are therefore impossible, and, when it is required to work free from shocks, the valves have only to be made heavier. During the stroke of the piston, heavy valves are suspended in the water, without striking the limiting stop. In general, the pumps which work best are those of which the valves have a small rise, and a large sectional area of passage-way, and which consequently float in the water without reaching the limit of lift. Such valves are favourable for great velocities. The high-speed draining-pumps of the Friedrichsthal and other mines are specially mentioned. They make usually from 40 to 50 revolutions per minute, and even upwards of 100 revolutions, equivalent to a speed of piston of 600 feet per minute. They are fitted with multiple-seat valves, and yield perfectly rectangular indicator diagrams.

The Paper concludes with a notice of Mr. Riedler's researches on steam-engines for drainage. He maintains that receiver compound engines working pumps at high speed are the real engines of the future.

D. K. C.

Roux's Water-Column Pump. By ALBERT MARNIER.

(Revue Industrielle, vol. xv., 1884, p. 114.)

The complete apparatus consists of a pair of double-acting pumps, one placed on each side of a large air vessel with which it communicates. Each pump consists of—

(1) Two horizontal gun-metal plungers of equal diameter separated by a larger piston. Upon the annulus formed by the difference of these diameters the motive power acts.

(2) A large piston-valve acting as a distribution-valve, and permitting the water to enter the annular space.

(3) A smaller piston-valve, which regulates the supply of water necessary to actuate the larger distribution piston.

(4) A reservoir in communication with the feed-water for driving the pump.

The action is easily understood. The large driving-piston makes a stroke, and at its extreme position causes the pressure of water to act upon the smaller piston valve, so that this in turn allows the larger one to take a stroke, and so admits the feed-water to the opposite end of the annulus. The exhaust is simultaneously opened so that the return stroke is made. It will be noted that there are no connecting rods, no external working parts, the water itself serves as a lubricant, and the action is noiseless.

The working conditions of the particular pump described, which is installed at St. Pierre, are as follows: The St. Pierre well is 267 metres in depth, and in this according to the season there accumulates from 1900 to 3000 cubic metres of water, which falls a height of 85 metres. The column to be raised is therefore 352 metres. It is found that a tenth part of the falling water can be raised through this height by the fall of the whole quantity through the 85 metres. The efficiency of the pump as a motor is therefore 41·4 per cent. the total work varies from 25 to 39 HP., the useful work from 10·4 to 16·3 HP.

H. S. H. S.

The Dalken Injector.

(Annales Industrielles, May 4, 1884, column 561.)

The Dalken injector is fixed directly to the front of the boiler, in such a manner that the delivery pipe advances direct into the interior of the boiler for a length of two or three feet; and that the steam pipe, also within the boiler, fetches a supply of steam from the most elevated point of the crown of the boiler, in order that only dry steam may be supplied to the injector. Then two tubes, free from the stress of unbalanced steam-pressure, may be made of thin copper. As the return to the boiler is short and direct, loss of heat is prevented. By this injector water can be lifted a height of from 5 to 7 feet. A table of quantities for different sizes is given in the Paper.

D. K. C.

Rock-Drill, by Dubois and François.

(Annales Industrielles, March 30, 1884, column 398.)

The new rock-drill of Dubois and François, known as the "Bosseyeuse," or bruiser, consist as usual of two parts, the perforator and the carriage. Three leading movements are provided for in their construction, namely, percussion, rotation, and advance.

The perforator consists of a cylinder, in which a piston, about 4¾ inches in diameter, is moved by compressed air, the cutter being carried on the end of the piston-rod; self-acting distributing gear, moved by the agency of the piston-rod; self-acting rotating gear; and advancing gear, moved by hand. The whole apparatus is supported on the carriage, on which it can be shifted and inclined at will. The supply of compressed air is distributed by means of two small piston-valves on one spindle, of unequal diameters, in the valve-chest above the cylinder. The excess pressure on the larger of these two pistons carries the slide forward and opens the port for compressed air behind the main piston, by which the cutter is launched against the rock. Meantime, through a very small passage drilled within the larger piston-valve, compressed air passes to the other face of this valve, and, acting on this, the face of larger area, drives the slide-valve, with its two pistons, to the other end of its travel, thereby exhausting the air behind the piston, and admitting compressed air to the front face of the piston. The return-stroke of the piston is thereby made, and the piston is placed ready for another forward stroke. The compressed air that had accumulated against the front face of the larger piston-valve is set free, when the slide-valve has nearly completed its return travel, by a small conical valve, opened by a pedal which is acted on by a bulbous expansion formed on the piston-rod, in the course of its return-stroke.

The rotation of the tool-holder, formerly effected by means of a helicoidal groove cut in the piston-rod, is now effected by means of a ratchet-wheel on the cutter-spindle: the spindle being free to slide within the wheel, and is made with two feathers which take into slots inside the ratchet-wheel. The alternate action of the ratchet-pawl—its pushing and return movements—is effected by means of two small upright pistons, under which, alternately, compressed air is admitted simultaneously with the admission of air behind and before the main piston. The pawl acting on one of these by pistons, moves the ratchet-wheels, say one tooth for each stroke of the cutter, and correspondingly causes the turning of the piston-rod and cutter; by the action of the other piston, the pawl is withdrawn in preparation for the next advance.

The advancement of the perforator as the hole is deepened, as well as its withdrawal, are effected by hand, by means of a longitudinal screw in a nut fixed to the cylinder. The cylinder itself is supported and guided by two parallel bars.

These two bars form part of the carriage, and are solidly framed together. They are supported on a truck, which is movable on a line of rails of about 2 feet gauge, having six wheels, of which one pair is placed behind, and two pairs in a frame on a pivot in front, making a conveniently flexible wheel-base about 3 feet in length. When the machine is placed for action, the truck is blocked before and behind, and a firm base is secured. The perforator, with the two longitudinal bearing-bars, are balanced by an adjustable counterweight at the back, and it may be manœuvred with rapidity. It can be directed to all points of the heading by a combination of the movements, horizontal and vertical. The vertical movement is effected in two ways, angularly by means of an endless screw and worm, and directly vertically by means of an upright screw bearing on the central bearing pivot. The horizontal movement is effected by means of a horizontal toothed segment and a worm, by which the apparatus is turned on the central pivot.

The Bosseyeuse measures about 2½ feet wide and 4 feet high, and weighs about 3 tons. It can work on lines of rails of from 18 inches to 3½ feet in gauge.

When explosives are used with the perforator in excavating galleries, a deep incision or slot, either vertical or horizontal, is cut into the face of the heading, near the middle. For this object two holes, about 2½ inches in diameter, and at a distance apart defining the extent of the slot, are drilled, and are filled with wooden pegs, roughly shaped. The slot is then cut out by the use of the cutter traversing to and fro in the manner of a slot-drill, between the defined limits. Thus a slot of from 2¼ inches to 3½ inches wide is cut.

In hard rock, the slot is formed by boring a series of round holes close together, then cutting out the intervals by means of a flat-toothed cutter. In this case, the rotating movement of the drill is not employed.

The slot having been cut, a number of holes are drilled on each side of the slot, and those which are nearest to the slot are the first to be charged and exploded. A slot of 4 feet or 5 feet deep, sufficient for an ordinary galley, can be cut in an hour in coal schist, and in less than two hours in sandstone. The subsequent cutting of holes in the neighbourhood of the slot occupies about double those times. This method of boring is the most economical in boring, as well as in explosives.

It has been proved by experience that the rate of advance is greater with the Bosseyeuse than with an ordinary perforator of four cutters, with economy in cost. Tables of comparative performances and costs are given in the Paper.

D. K. C.

Hock's Hot-Air Engine.

(Annales Industrielles, May 4, 1884, column 559.)

Mr. Hock, of Vienna, has produced a hot-air engine of 30 HP., in which the fundamental notion is the forcing of regulated quantities of air by means of a compressor, through the fire-grate and burning fuel of a furnace, where it serves in part to maintain combustion, and the rest is mingled with the gaseous products, and is conducted to a working cylinder, in which the mixed gases act upon a piston. To obviate excessive wear, a certain proportion of water is discharged among the hot gases, and evaporated by and mixed with them on the way to the working cylinder. Thus, whilst the temperature is reduced, there is generated, in compensation, a quantity of steam-gas.

The engine consists essentially of the furnace, the working cylinder, and the compressor. The furnace is constructed of sheet-iron, and can be hermetically closed. It is lined with firebrick. Fuel is introduced through the upper part by means of a double-valve movement. Two openings are made into the furnace, one above, and one below the grate, which can be hermetically closed. An escape-pipe for the gaseous products is provided. In lighting and heating up, the openings and the pipe are open, the gaseous products being discharged direct into a chimney. When the furnace is sufficiently in working order, the openings and the tube are closed, and a little water is introduced, which, immediately evaporating, starts the engine. Air is supplied by the compressor, from which it is delivered in two branches, one below the grate, and the other at the upper part of the furnace. The air passed through the grate is partly consumed in combustion; and the unconsumed air passes up at a high temperature with the burnt gases into the upper part, where the currents meet the air introduced above. Complete combustion of the fuel is thus ensured, and the combined current modified by the water injected and evaporated passes to the working cylinder. The working-pressure is three atmospheres, and the gases are exhausted at a pressure of one and a half atmosphere. The compressor-cylinder is placed below the working cylinder, and its piston is moved by a prolongation of the working piston-rod. The engine is regulated by a governor.

From the results of tests, it appears that from 2·20 lbs. to 1·65 lb. of coal have been consumed per HP. per hour.

D. K. C.

Hassa's Plank Hammer.

(Annales Industrielles, June 1, 1884, column 678.)

In Hassa's hammer, constructed by Messrs. Emile, Jules Halot and Co., Brussels, the hammer-rod consists of a plank of wood, which is embraced between and is lifted, and sustained at will, by a pair of friction-rollers. Hence the hammer is sometimes called the plank-hammer.

The hammer consists of a frame bolted to a base-block or anvil-block, with a piece of card-board between them. The base-block is bolted down to a timber foundation, consisting of two longitudinal pieces, reposing on and bolted to two transverse pieces, which are bedded in concrete. The hammer or tup slides between guides on the frame; the hammer-rod is constructed of three board superposed and solidly glued together, of which the middle board is of ash, and the two outer boards of elm. They are, in addition, pinned together by numerous pins of ash fixed with glue. The plank is planed on both sides, to a thickness of 1 inch at the lower end, and 1⅜ inch at the upper end. The lower part of the plank is thickened up to 1½ inch, where it is fixed into the hammer as an inverted wedge, with an iron plate fixed with set-screws. The friction-rollers are of cast-iron, turned exactly to the same diameter. They are in continuous motion, in opposite directions, each being driven by a separate belt-pully, at a speed of 120 revolutions per minute. The frictional adhesion necessary for lifting the plank by the rollers is effected by pressing the rollers towards each other. This is done by means of eccentricated bearings by which the roller-shafts are carried, which can be adjusted by hand to give the required pressure.

Four sizes of plank-hammer are manufactured, having hammers respectively, 2 cwt., 3 cwt., 4 cwt., and 5 cwt., and using from 1 HP. to 3 HP. The plank-rod lasts from 15 to 18 months.

D. K. C.

On New Forms of Swedish Hearth- and Welding-Furnaces.

By G. A. Forsberg.

(Jernkontorets Annaler, 1883, p. 360.)

The Author describes a modification of the Swedish "Lancashire" hearth finery, which has been introduced at the Skutskär ironworks. The hearth is of the rectangular form usual in Sweden, but differs from the ordinary construction in having a third twyer in the back wall, that opposite to the working or front side. The side walls are formed of hollow iron castings, which are cooled by allowing the blast to pass through them, whereby it becomes heated before arriving at the twyers. The hearth is covered by an arched roof of brickwork, carrying a hopper closed by a slide, in which the charcoal is charged and dropped into the fire as required, instead of the old plan of shovelling it in through the working door in front. The waste flame, on its way to the chimney, passes over a bed in which the pig iron for the following charge is brought up to a strong heat before its introduction into the melting-hearth.

A comparative trial of one of these hearths against one of the ordinary kind with two twyers, carried on for a period of four weeks, gave the following results.

The two-twyer hearth produced an average of 265 cwt. of blooms per week, with a consumption of $5\frac{2}{3}$ bushels of charcoal per cwt.

The three-twyer hearth produced 366 cwt. per week of bloom-iron, with a consumption of $4\frac{3}{10}$ bushels charcoal per cwt.

The loss in the metal in working was about the same in both cases, namely, 12·93 per cent., or 1·1485 cwt. of pig-iron were required to make 1 cwt. of bloom-iron.

The Author suggests a method for further increasing the production of this class of furnace, by doing away with the melting of the pig-iron in the refining-hearth. For this purpose the heating-bed for the metal in the flue is to be converted into a melting-bed, which is kept warm by the waste flame during the actual firing period, and receives the heat necessary for melting from a gas-producer, placed at right angles to the working axis of the hearth. This arrangement is figured in the plate illustrating the Paper. Two hearths, placed back to back, communicate with the same stack, and are served by a producer common to both. The gas being only required during the melting period, the producer is damped during the refining of the metal in the hearth. The gas-producer may be worked with inferior fuel, in which case the melting of the pig is done at less cost than in the fining hearth, where charcoal must be used.

The welding-furnace described by the Author is a gas-furnace, with a flat bed about 20 feet long, and low arch, standing within its own blast-heating apparatus, the latter being a series of short upright and horizontal pipes, the former being also part of the structural ironwork of the furnace. The horizontal pipes are placed below the bed, and correspond in some degree to the long horizontal air-way in Bicheroux's furnace. The gas-producer has a stack 10 feet high, shaped somewhat like that of a charcoal blast-furnace, the bottom being closed by a grate arranged to work upon a central axis. The charging is effected by a gas-tight covered hopper and valves. The gas passes by a siphon up-take and down-come within the same mass of brickwork as the stack, to a chamber forming the ashpit of the furnace, where it meets the heated air in jets from a square pipe in the roof, a short distance in front of the fire-bridge. The producer is worked with waste short ends of deals and planks, previously dried at 104 to 150° Centigrade, the consumption being equivalent to 2·7 bushels of charcoal, supposing the wood to be equal to 63 per cent. of the latter. The loss upon the blooms heated for rolling into merchant iron was 11·5 per cent. A further modification of this furnace is provided with two similar gas-producers, which are worked alternately, the fuel charged wet in one being dried while the other is being converted into gas. For the former purpose the gas-delivery passage is closed by a valve at the bend of the gas-delivery siphon, and the charging-valve of the hopper is opened while heated air from the hot-blast tubes is passed through the wet wood from below, until hygroscopic water is converted into steam.

H. B.

Charcoal-Burning in Kilns at Boxholm, Sweden.

By W. Sandahl.

(Jernkontorets Annaler, 1883, p. 307.)

At Boxholm, in Sweden, kilns have been introduced for the purpose of carbonizing saw-mill waste for ironworks. They are rectangular chambers, 30 feet long, 20 feet broad at the bottom, 18½ feet at the spring of the semicircular roof, and 19 feet high to the top of the latter. The side walls, 11½ feet high, are made with a slight inward fall. The floor is flat, except at the end walls, where it is laid with a small slope towards a cast-iron pipe, giving issue to the gases and condensable products. The former escape by an upright pipe made of sheet brass, while the latter are collected in a wooden cistern, forming a tar well. A rectangular fireplace, lined with firebrick, extends under the centre of the floor for about half its breadth, where it is connected with five narrower flues, branching out to the four corners and the centre of the opposite long side, through which air is admitted to the chamber. The necks of these flues are covered by iron pipes, with low conical cowls projecting about 9 inches above the floor-level. The arch is protected against rain by a light wooden roof. The saw-mill waste ribs and slabs are brought to the kilns by railway, and stacked to dry in the air until wanted. The charging is effected through doors, of which there are two, at either end of the kiln. One of these, at the floor-level, is 5 feet by 4 feet, while the other, at the level of the arch springing, is only 4½ by 2½ feet. A bed of logs or large ribs is laid across the kiln to a depth of about 9 inches, in order to facilitate the firing of the wood, and upon this the remainder of the charge is laid, with the length of the wood in line with the longer sides of the kiln. Experiments made with alternate layers, laid crosswise, gave no better results, while the charging was somewhat more expensive.

When the whole quantity of wood, about 10,000 cubic feet, has been charged, which can be done by ten men in three or four days, the lower doors are closed and bolted, and fire is applied through the central flues. The upper doors are left open until the third day, to give a free vent for the steam given off from the damp wood. This is succeeded by gas, at first white, and then becoming darker, and more abundant until the middle of the sixth day of burning, when it mostly changes to blue. The burning is controlled by the character of the gas; if the fire takes too strong a hold, the flues are damped or plugged with wood, to check it. To provide for the relief of internal gas-pressure upon the structure in the latter case, the side walls are perforated with three small tubes, about 18 inches above the floor, which are kept constantly open. According to the degree of dryness of the wood, the burning lasts from ten to eighteen days; the cooling takes sixteen days, and the discharging is effected in one and a half day by two men. When the boiler is emptied, any repairs that may be required are made, when the next charge is immediately introduced.

The coaling is done by piecework; the total cost for labour is about 1*s.* 4*d.* per last (76 cubic feet) of charcoal produced. The annual production of each kiln is 800 lasts of coal and 160 gallons of tar.

The loss in carbonizing varies with the dryness of the wood, but may be taken as averaging 1,000 feet upon the total contents of 10,000 feet, the volume of the coal obtained being from 85 to 90 lasts, or 6,460 to 6,840 cubic feet, representing a yield of 58 to 62 per cent. in volume of the wood used.

When the ground occupied by the kilns was used as an open coaling place, from two to three piles of 75 lasts could be burnt annually, with a cost for labour of 2*s.* 5*d.* per last, with a yield of 50 per cent. upon the volume of the wood burnt. The coal produced from sawn stuff is less adapted for blast furnaces than for Lancashire forge fires, for which it is well suited when mixed with a proportion of charcoal from natural wood.

As compared with open heaps, kilns have the following advantages:—

1. Lower working cost.
2. Better yield, the charcoal being cleaner and freer from dust.
3. The carbonization is more easily conducted not being dependent upon the weather, and also requiring less skilled labour.

H. B.

On the Influence of Punching on Soft Steel.

By W. Beck-Gerhard.

(Gorni Journal, St. Petersburgh, March, 1884., p. 347.)

During the course of experiments made at the Poutiloff works at St. Petersburgh, on the influence of punching on the strength of soft steel fish-plates, the results, already well known, were arrived at, namely, that punching in the cold weakens perceptibly, and reduces the elongation of the steel; that steel annealed after cold punching, and when punched hot, is not injured; that annealing, when well done, even increases the tenacity of the punched specimen, and that the evil effects of cold punching are in a great measure removed by subsequent rimering out of the hole.

On bending samples of soft steel which had been punched in the cold through the holes, it was invariably found that the specimen would bend without cracking, if the punch side of the hole was on the convex side of the bend, but if the bar were bent in the opposite direction, that is to say, with the die side convex, the specimen broke. This was the case with all cast metal; while puddled or scrap iron could be bent without injury. This phenomenon led the Author to institute an investigation as to whether there was any foundation for the very generally received opinion that the edges of a punched hole on the die side are injured by a ring of minute incipient radiating cracks. For this purpose a large number of specimens, 5 inches by 3 inches by ½ inch of all kinds of steel were prepared. The edges were planed, the surfaces polished, holes were pierced in various ways, and the metal surrounding them was carefully examined with a microscope, but no trace whatever of cracks could be found, though the nature of the steel ranged from 0·1 to 0·6 per cent. of carbon.

Although the search for incipient cracks proved fruitless, Mr. Beck-Gerhard observed and has described, he believes for the first time, certain markings on the polished surfaces of the plates around the cold punched holes, which seem to possess very great interest. Visible to the naked eye, and surrounding the holes, were sheaves or bunches of lines starting tangentially to the holes, and curving slightly towards them. These lines branch out in opposite directions, and intersect with some degree of regularity. They do not appear in the vicinity of drilled holes, but are distinct in cold-punched holes rimered out. In forged iron they did not appear, although they were most distinct in the softest steel, and vanished when the metal reached the hardness due to 0·6 per cent. of carbon. An increase of thickness in the plate caused a corresponding increase in the number and clearness of the lines, upon which the shape of the hole was also found to have an effect. In all the 5 inches by 3 inches specimens the lines were distinctly sunk, while in two of the 10 inches by 10 inches specimens, the lines were prominent, sinking gradually to the level of the plate towards their ends. At the points of intersection near the holes the continuity of the lines was interrupted, dots or nodes representing their course.

In the 3-inch specimens with planed edges the lines terminated abruptly at the edges, but in natural bars, such as fish-plates, the more pronounced rays turned round the edges, and actually appeared on the opposite side. In all the samples the lines were less developed in the upper or punch side of the plate, and even at times degenerated into a mere frosted appearance. Heating the plates to redness did not obliterate the rays, though it rendered them less pronounced.

In order to determine how far the rays extended around a cold-punched hole, a 10-inch square sample was cut out of a ⅝-inch steel plate, the surfaces were polished, and a 1-inch hole was punched in the centre. The curved rays appeared very strongly marked on the die side, and less pronounced, but still very distinct on the punch side. The surfaces of the plate were then washed with aqua regia, when the rays disappeared, but the surfaces became streaked with elongated bubbles and hair-lines, arranged in the direction in which the plate had last passed through the rolls. The sample was then cut into eight test-pieces, four on each side of the central hole, and subjected to rupture by tension. The result was an average ultimate strength of about 27 tons per square inch, with an elongation of 20 per cent. All the fractures took place at one of the elongated bubbles, and the polished surfaces of the specimens developed ridges and indentations, which could not only be seen but felt. On placing the specimens together in their original relative positions as a 10-inch square plate, it was at once seen that the markings on the strips formed together a system of curved rays around the central hole, precisely analogous to those which the solid plate had exhibited, only the rays extended much further over the surface. Test-bars cut from a similar $\frac{5}{8}$ inch plate 10 inches square, which had not been punched, exhibited none of the marks above described; hence it must be concluded that the effect or influence of the 1-inch cold punched hole had extended all over the 10-inch plate. The observations made so far, the Author considers, are insufficient for the foundation of a theory, and regrets that his occupations do not admit of further investigation.

W. A.

Remarks on some New Observations on the Working of Steel.

By D. CHERNOFF.

(Imperatorskoye Russkoye Tekhnitcheskoye Obschestvo, March 10, 1884.)

The Author, whose name is already well known in connection with the working of steel, read a Paper before the Imperial Technical Society of St. Petersburgh on the phenomenon observed by Mr. Beck-Gerhard (*ante*). He stated that seven or eight years ago he had noticed, in cold-sheared samples of steel subjected to a destructive tensile strain, that when the limits of elasticity were reached, the scale on the specimens began to separate in a peculiar manner, and the surfaces of the samples were marked with curved lines of more or less regularity. He made arrangements to institute an exhaustive inquiry into the meaning of these lines, but unfortunately circumstances obliged him to abandon his intention until the commencement of the present year, when Mr. Beck-Gerhard invited him to examine his specimens, and awakened once more his desire to investigate the subject. The appearance of the rays on the polished surfaces of the steel reminded Mr. Chernoff of the experiments made by Mr. Leger (Sur la constitution des corps trempés. Mémoires de la Société des Ingénieurs Civils, 1877, p. 645), in which the lines of strains produced in glass subjected to various pressures were made visible, through the circumstance of the light passing through the specimens becoming polarized in the regions strained, and therefore capable of being made manifest by being examined through a Nicol prism. Similar observations were recorded in the year 1877 (Société des Sciences Industrielles de Lyon. Séance du 25 Juillet, 1877), and it occurred to Mr. Chernoff that, as in glass-bearers, the lines of strains assumed various curved forms, so, in metal, similar actions took place, and became, in part, manifested on the surface so soon as the limits of elasticity were passed. In glass the elastic limit coincides with that of ultimate strength, so that it is impossible to fix the waves of strain, but it is worthy of remark that the forms assumed by the fragments of conchoidal fractures resemble closely the lines of strain revealed in glass by the Nicol prism. In metals, on the other hand, the permanent set will begin first where the strains are greatest, and hence the deformation of surface will follow the lines of maximum strain.

Why the regions of maximum strain should arrange themselves in the form of curved rays in metals is as difficult of explanation as the corresponding phenomena in glass; but Mr. Leger suggests that the propagation of strains through elastic substances may be of an undulatory nature comparable to the propagation of sound.

Mr. Rgeshotarsky, one of the officers employed at the Abouchoff Steel Works, undertook to repeat and extend Mr. Beck-Gerhard's experiments, and from his observations it was found that the lines of strain were manifested not only in punching, but in shearing, and in flattening under the steam-hammer. From these experiments the analogy between the visible lines of strain in steel and in glass and sound-waves was rendered very striking, because the lines of strain from shearing and punching in the same specimen, and from the two points of compression under the hammer seemed to pass each other without mutual hindrance, except in so far that at the points of intersection the phenomenon of interference was clearly observable. In order to confirm the supposition that shearing produced the same effects as punching, Mr. Rgeshotarsky polished the surface of a ½-inch plate, which had been sheared round its edges, and subjected it to tension beyond its elastic limits. The curved rays emanating from the two sheared edges appeared very distinctly intersecting each other, and were sufficiently prominent to affect the sense of touch. It was noticed by Mr. Beck-Gerhard that the rays are of two kinds. One set, produced by tension, are depressed below the surface, the other due to compression, are raised. What kind of rays should be expected from a specimen which had been sheared, then polished and finally stretched?

Hodginson long since noticed that when iron bars, stretched beyond their elastic limits, were again subjected to tension, that the elastic limit was considerably raised; and the same fact may be deduced from the well-known circumstance that drawn iron wire becomes harder and more rigid as it passes through the dies. In the same way, therefore, in the sunk rays produced by the tension due to a shearing force, the metal has been stretched beyond its elastic limit, and consequently that limit has been raised. After obliteration by polishing, when direct tension is applied, the metal in the rays is last to yield, that between them is extended, and forms hollows, leaving the rays prominent. Such is the observed effect, although at first sight it appears paradoxical. Mr. Chernoff acknowledges his inability to discuss the question of the curvature of the lines of maximum strain, but he draws attention to the similarity between the curved lines of fracture in a broken pane of glass and the rays developed by punching and shearing steel. He remarks that the slower the action of the force that breaks the glass the more extensive is the injury, while, under rapid impact, such as from a bullet, hardly any radiating fractures are formed, and he infers that similar results would follow in the case of metal plates, acted on with different degrees of rapidity.

It would be a very useful undertaking, with reference to the strength of guns, and especially of ordnance of large calibre, to investigate the effects produced on the inner tubes by the pressure due to the successive tiers of hoops. In spite of very elaborate formulas for calculating the compression produced on the inner tube by the successive layers of rings, no information exists as to how the strains are distributed in the metal of the tube, because all observation leads to the conclusion that the strains are not transmitted in the regular way assumed by the formulas, but in a wave-like progression forming bands of irregular tension and consequent plains of weakness. Did not the existence of such plains of weakness explain the anomalous behaviour of inner tubes, which, though made of excellent metal, carefully tested at every stage of its manufacture, yet crack in an unaccountable manner after more or less prolonged firing?

This paper, as well as the foregoing, is illustrated by numerous plates, for the most part engraved from photographs taken from the specimens.

W. A.

On the separate Ventilation of Coal-Workings.

By — Von Steindel.

(Jährbuch für das Berg- und Huttenwesen (Freibeg), 1884, p. 78.)

The mines of the Zwickau-Oberhohndorf Coal-mining Company, to which this communication refers, are provided with a Guibal fan of 9 metres in diameter. This would be sufficient for all purposes of ventilation, if the ground were sufficiently solid to allow of air-courses of large area being used. As it is, however, the drag on the air is so great, that it is not considered desirable to diminish the strength of the main current by a complicated system of splittings and crossings; and therefore a method has been adopted for providing for the ventilation of all ends and workings not lying on the main air-course, by a special supply of air sent down from the surface under pressure. For this purpose air-compressors have been established at the two principal shafts, each of which, when making forty strokes per minute, compresses 31 cubic metres of air at the ordinary pressure to 3 atmospheres above it. The delivery-pipes are united underground, so that either of the two compressors may command the work in case of the other being laid off.

The number of working-places requiring ventilation being large, forty-six in all, it was impossible to use the compressed air indirectly, *i.e.*, to drive fans, or other blowing machines, and the trials were confined to two methods, the first, and simplest, by that of allowing the air to flow into the working at full pressure from the end of the delivery-pipe; and the second, that of using a jet-aspirator similar in principle to Körting's apparatus.

With the direct efflux the following results were obtained:—

Diameter of Nozzle.	Volume of Air at Atmospheric Pressure delivered at the Absolute Pressure in the Main of		
	2 atmospheres.	3 atmospheres.	4 atmospheres.
Millimetres.	Cubic Metre.	Cubic Metre.	Cubic Metre.
1·5	0·116	0·162	0·216
2·0	0·162	0·204	0·277
3·0	0·273	0·439	0·558
4·0	0·477	0·620	0·816
5·0	0·612	0·920	1·224

The maximum volume of air, therefore, obtainable by this method, does not exceed 43 cubic feet per minute, and the whole work of one compressor would be required for the ventilation of twenty-two working-places. The dispersal of fire-damp was very rapidly effected, but the noise of the escaping air was so great as to overcome all other sounds, especially the premonitory cracking of the roof before falling—a defect that rendered the use of such a method of ventilation exceedingly dangerous. The experiments with the aspirator were made by allowing the 1·5-millimetre jet of the apparatus (which is not described in detail), to flow into a zinc pipe 150 millimetres in diameter, and 10 metres long. The results obtained were:—

Absolute Pressure in Main.	Volume of Air at Atmospheric Density.	Velocity in Metres per Minute.
	Cubic Metres.	
2 atmospheres	2·240	128·0
3 "	3·019	172·5
4 "	3·798	217·0

The air delivered is therefore at 2 atmospheres 19·3 times, at 3 atmospheres 18·6 times, and at 4 atmospheres 17·6 times, that of the compressed air consumed. When pressure was increased from 2 to 3 atmospheres the volume delivered increased 34·8 per cent., and from 2 to 4 atmospheres 69·6 per cent. The velocity of the current was in all cases sufficient to effect the rapid dispersal of fire-damp. The next series of experiments were carried out with a simplified jet-apparatus. A gas-pipe of 17 millimetres, connected with the blast-main, and terminating in a conical nozzle of 1·5-millimetre aperture, was inserted in the 150-millimetre zinc pipe to a distance of 500 millimetres from one end. This gave much better results than the preceding, as will be seen in the following Table:—

Absolute Pressure in Main.	Air Delivered at Atmospheric Volume.	Velocity in Metres per Minute.
	Cubic Metres.	
2 atmospheres	3·623	207
3 „	5·163	295
4 „	6·143	351

In the first case the useful volume of air delivered is 31·2 times, in the second 31·8, and in the third 38·4 times that of the air expended. The fourth series of experiments were made by varying the size of blast nozzle from 1·5 to 2, 3, 4, and 5 millimetres, and the diameter of the zinc pipe from 150 to 210 and 310 millimetres, the length of the latter being in all cases 10 metres.

Diameter of Nozzle.	Zinc Pipe.	2 atmospheres.		3 atmospheres.		4 atmospheres.	
		Air Delivered at Atmospheric Volume.	Velocity in Metres per Minute.	Air Delivered at Atmospheric Volume.	Velocity in Metres per Minute.	Air Delivered at Atmospheric Volume.	Velocity in Metres per Minute.
Millimetres.	Millimetres.	Cub. Met.		Cub. Met.		Cub. Met.	
1·5	150	3·623	207	5·163	295	6·143	351
2·0	„	4·865	278	6·668	381	8·628	493
3·0	„	6·633	379	9·380	536	11·778	673
4·0	„	8·838	505	12·250	700	14·438	825
5·0	„	10·605	606	14·158	809	17·623	1,007
1·5	210	5·390	154	7·735	221	9·205	263
2·0	„	7·035	201	10·360	296	12·565	359
3·0	„	9·205	263	14·175	405	16·345	467
4·0	„	12·005	343	18·445	527	21·560	616
5·0	„	15·680	448	21·315	609	26·250	750
1·5	310	7·800	104	10·875	145	14·100	188
2·0	„	11·850	158	15·300	204	18·300	244
3·0	„	14·925	199	21·300	284	25·275	337
4·0	„	20·250	270	27·600	368	33·675	449
5·0	„	24·525	327	32·100	428	41·250	550

The results obtained in the preceding experiments showed that the method of ventilation by compressed air is as capable of being practically applied on the large scale, but before adopting it further experiments were made to determine the loss of effect due to the use of long lines and pipes between the points of aspiration and delivery of the air. For this purpose experiments were made in three different kinds of workings, namely, a dip-heading 80 metres long, an inclined plane 120 metres, and a cross-cut gallery of 200 metres. In all cases the maximum absolute pressures of four atmospheres was used, that being found to be the most advantageous for practical work. The results obtained showed that sufficient ventilation might be obtained, even at a distance of 200 metres from the jet. The loss of effect is, however, very considerable. Thus, with a 3-millimetre nozzle blowing in a 210-millimetre pipe 10 metres long, the air delivered per minute was 16·345 cubic metres, but when the length was increased to 80 metres, it was diminished to 10·395; at 120 metres it was 8·680 cubic metres, and at 200 metres 5·600 cubic metres, or only about one-third of the delivery of the 10-metre pipe. The loss of air upon the quantity actually drawn into the pipe is, however, comparatively small even on the long length. In the case of the 200-metres length given above, the air delivered was 65·5 per cent. of the combined volume of compressed air expended, and that at ordinary pressure drawn in by the jet. When, as is customary in the mines, rectangular wooden air-pipes of 285 × 420 millimetres internal section are substituted for the round zinc ones, the loss is considerably greater, the volume delivered being only 44·28 per cent. of that aspirated. The wooden pipes are used in preference to those of zinc, as not being liable to be crushed when the thrust of the ground is heavy.

Further experiments were made on the long lines to see how far it was possible to divide the current so as to ventilate two or more points from one jet. These showed that it was possible to ventilate as many as four points from one blower, but the latter seemed to be rather too large a number, and that it was more advantageous not to divide the current more than once. In a final Table a list is given of the different workings where this method of ventilation was in use in May 1883. There were forty-six in number, with more than 2000 metres of pipes of varying dimensions, the longest line being 235 metres. The total delivery is about 430 cubic metres per minute, the velocity varying from 37 to 390 metres per minute. The number of workmen supplied with air was 96. In cases of sudden outbursts of gas the working place may be rapidly cleared by removing the nozzle, and allowing the compressed air to flow from the full aperture of the pipe.

In a final note the editor calls attention to a similar use of compressed air made by Weigel at Lugau in 1881.

H. B.

On Russell's Process for the Lixiviation of Silver Ores.

By C. A. Stetefeld.

(Proceedings of the American Institute of Mining Engineers, Cincinnati Meeting, May 1884.)

The extraction of silver by lixiviation from ores that have been chloridised by washing depends upon the solubility of silver chloride in solutions of sodium or calcium hyposulphite, and the conversion of the dissolved silver into insoluble sulphide by the addition of an alkaline sulphide with the regeneration of the hyposulphite salts. When lead is present in the ore, a large proportion also goes into solution, lead sulphate being soluble in hyposulphite solutions; and copper, if present, is also precipitated by sulphide, so that the resulting metallic precipitate contains lead and copper in addition to silver, giving a combination not desirable for subsequent treatment.

The new process invented by Mr. E. H. Russell, of Park City, Utah, involves some important reactions not previously described. These are—

1. Lead can be completely separated from a sodium hyposulphide solution as carbonate by sodium carbonate or purified soda ash, without precipitating either copper or silver, which, remaining in solution, are obtained as sulphides in the usual way, except that sodium sulphide must be used as a precipitant. If calcium polysulphide were used, the lime salt found in the regenerated lixiviation solution would subsequently go down as carbonate with the lead on the next addition of soda-ash. The experiments of the inventor have, however, shown that sodium sulphide and hyposulphite are more efficacious in the extraction of silver than the corresponding calcium-salts.

2. The solution of a double hyposulphite of copper and sodium, formed by mixing sodium hyposulphite with copper sulphate, exerts a powerful solvent and decomposing action upon metallic silver and its sulphides, sulpharsenides, and sulphantamonides; whereas the simple alkaline hyposulphite solution is only efficacious in the complete extraction of silver chloride. If, therefore, a charge of ore is first lixiviated by sodium hyposulphite to remove the silver chloride, and subsequently with cuprous hyposulphite, the solution of the latter salt being called by the inventor the "Extra solution," an additional amount of silver is extracted, which would have been lost in the tailings when worked according to the old method.

The process may also be used to extract silver from ores without roasting, or after they have been subjected to an oxidising roasting without chloridizing.

The sulphides of silver and copper obtained as precipitates in the lixiviation process are dissolved in nitrated sulphuric acid, the escaping nitrous fumes being reconverted into nitric and nitrous acid, in the usual way. From the solution silver is precipitated by metallic copper, the copper sulphate being recovered by crystallization. A portion of the latter is again used in preparing the extra solution.

The bulk of the Paper contains numerical details of the experiments made by the inventor, which should be consulted, as they are too full to be reproduced in abstract.

The process has been tested on the large scale at the Ontario Mill, in Utah, with a plant capable of lixiviating charges of three tons in one tank. In these experiments the following facts have been ascertained:—

1. *Preliminary Leaching with Water.*—Before turning on the hyposulphate solution, it is more judicious to use cold rather than hot water, as a large proportion of silver chloride would be dissolved by the latter. The first wash-water, containing copper and some silver, is conducted to tanks filled with scrap iron, where the copper and most of the silver is precipitated. Mr. Russell has determined, upon ten charges of 2 tons each, the total amount of silver dissolved by the wash-water to be 0·39 oz. per ton, and that recovered in the cement copper, 0·28 oz., or a loss of 0·11 oz. per ton.

These figures apply only to the particular case, as they depend upon the amount of undecomposed salt remaining in the charge.

2. *Lixiviation with ordinary and Extra Solutions.*—The first extraction was effected with ordinary sodium hyposulphite solution containing 1¼ to 1½ per cent. of the commercial crystallized salt. It is made slightly acid with sulphuric acid to neutralize any caustic soda that it may contain. When the bulk of the silver chloride is dissolved, the extra solution is turned on to act upon the other silver compounds. This method of lixiviation has the best effect, as silver chloride is less easily soluble in the extra than in the ordinary solution, the solubility decreasing rapidly with the proportion of copper sulphate added.

The standard extra solution is made by dissolving 18 parts of sodium hyposulphite and 10 parts of copper sulphate in a small bulk of water, mixing the solutions, and allowing the double salt ($2\,Na_2\,S_2\,O_3\,3\,Cu_2\,S_2\,O_3 + 5\,H_2O$) to separate.

The clear liquor containing sodium sulphate and tetrathionate is decanted, and the precipitate, after washing, is dissolved in a solution containing from 1 to 2½ per cent. of pure sodium hyposulphate. This extra solution of constant strength is used for several ore charges in succession until the copper salt is nearly exhausted before it is turned into the precipitating tanks.

3. *Separation of Lead.*—This requires an extra series of tanks. The soda-ash solution requires to be purified from sulphide and caustic soda. This is done by adding to the watery solution of soda-ash about 1½ per cent. of sodium hyposulphite and some sulphate of copper. The sulphur compounds are decomposed, giving insoluble sulphide of copper, while the copper carbonate formed remains in solution. The caustic alkali is previously converted into polysulphide and hyposulphite by boiling the solution with sulphur, the sulphide being then removed by copper as described above. The lead carbonate goes down rapidly, and in less than one hour the solution is sufficiently clear to be decanted into the silver precipitating tanks. When a sufficient quantity has accumulated, it is collected by a filter press. The value of the lead salt recovered may be sufficient to pay for the soda-ash used;

but apart from this, there is the advantage that the lead would otherwise have to be separated by an alkaline-sulphide, which is a more costly reagent. The great advantage, however, realised is the absence of lead in the subsequent precipitation of the lixiviation solution.

4. *Precipitation of Silver and Copper Sulphides.*—As has been previously stated, the process necessitates the use of sodium, and the exclusion of calcium sulphide as a precipitant. This involves the question of the relative advantages of the two compounds, which is discussed in very full detail. The inventor's experiments seem to show that the sodium sulphide solutions are preferable, as keeping their power unchanged for a greater length of time than those of calcium sulphide. The best precipitant seems to be obtained by boiling caustic soda with sulphur, which in some cases has a precipitating energy in excess of the theoretical limit depending upon the original concentration of the caustic soda solution. The question is, however, an extremely complicated one, which the inventor proposes to investigate in greater detail at a future time.

5. *Treatment of the Sulphides.*—The sulphides, when treated in the ordinary way by roasting and fusion, yield bullion of 874–876 silver fineness, but regulus with 24–27 per cent. of copper, and 2,720–3,300 ozs. of silver per ton is also formed to the extent of 62–76 per cent. of the weight of bullion obtained.

This mode of treating the sulphides is essentially unsatisfactory, owing to the low fineness of the bullion obtained, the large amount of regulus, and the losses of silver by absorption and volatilisation are very considerable. The Author has therefore devised a new treatment, the details of which are generally as follows:—

The sulphides, when collected by a filter-press of Dehne or Johnson's construction, are dissolved without previous drying in sulphuric acid, with an addition of nitrate of soda. No heating is required, the reaction being very violent. Gold, if present, remains undissolved, as does chloride of silver, which may be formed from chlorides in the sulphuric acid or nitre. The sulphur separates in globules, and is used again in the preparation of sodium sulphide.

After complete decomposition the solution is drawn into a tank, and the silver is precipitated by metallic copper. For this purpose, the cement copper obtained from the first wash-water may be advantageously used, as its silver is thereby recovered. It should, however, first be melted, refined, and cast into plates of the proper shape. The acid in the liquor is used in dissolving fresh sulphides. The sulphate of copper formed is crystallized, only a portion being required in keeping up the extra solution. The nitrous gases are regenerated by passing them through a coke tower, with water or sulphuric acid spray, as in sulphuric acid works.

Practical results obtained—

The average of twenty working tests with the Russell process upon charges of 2 tons each, showed—

Lixiviation test with		
Ordinary Solution.	Extra Solution.	Silver Extracted.
Per cent.	Per cent.	Per cent.
86·6	92·9	92·6

Seven charges recently worked gave—

90·0	93·9	94·4

With hyposulphite solution near the freezing point, the silver extracted averaged 7 per cent. above the chlorination assay with the ordinary solution. This shows the method to be efficacious at low temperatures, but it is more advantageous to use moderately warm solutions.

As regards bye-products, from 5 to 8 pounds of carbonate of lead are obtained per ton of ore; 3½ lbs. of copper per ton were saved in the wash-water. This copper contained about 130 ozs. of silver per ton.

If the sulphides are treated by the wet process, more than half of the copper could be used in precipitating silver. The remainder might be oxidized by roasting and converted into vitriol, leaving the silver in the insoluble residue. A part of the sulphate is kept in circulation in the extra solution.

The cement silver, after being compressed into cakes, is melted in a small reverberatory furnace with a gas-producer. In a furnace of this kind at the Lexington Mill, in Montana, wood is used as fuel in the producer. When fired at 4 A.M., the first cast of ten bars of 1,800 ozs. each is completed at 9 A.M., when the furnace is immediately re-charged, and a second cast is made at 11 A.M.; half a cord of wood being required in the first melting, and only a quarter in the second, the furnace being already heated. It was originally intended to use gas from charcoal, but that from wood is found to be better and cheaper.

A valuable feature in the process is the probability of the substitution of rolls for the stamps used in drying and crushing the ore previous to its treatment. Two sets of Krom's 26-inch steel rolls, costing $6,180, take the place of 30 head of stamps, costing $17,118; with a daily saving of $27·23 for the same amount of work done.

Upon a comparison of the process with amalgamation, to which it is considered to be superior both on economic and sanitary grounds, the Author considers that it is likely to effect a revolution in the methods of reducing silver ores, and a diminution in working expenses such as has been rarely witnessed in the history of metallurgy.

H. B.

The Extraction of Sulphur by Chloride of Calcium in Sicily.

(Berg- und hüttenmännische Zeitung, xliii., p. 277.)

In 1868 a method of extracting sulphur from its ore was patented in Italy by Dépérais, in which the ore was plunged into a solution of chloride of calcium, heated from 10 to 20 degrees above the melting-point of sulphur. This has only been advantageously used since the price of chloride of calcium has been cheapened by its production in large quantities as waste in the ammonia-soda process of Solvay, and it now costs, including carriage, 90 francs per ton in Sicily. The method has been adopted by Messrs. C. Vincent de la Tour and Dubreuil at the mines of Tronica, Grotta Rossa Pernice, near Racalmuto and Crocca, at nine different points. The plant consists of two rectangular wrought-iron boilers, each of two cubic metres capacity, which are heated alternately by one fire-place, lignite coke or coal being used as fuel. As some varieties of the ore disintegrate in the chloride of calcium solution and fall to powder, which, mixing with the sulphur, renders the latter impure, the boilers are provided with an inclined feeding trough, protected by vertical iron gratings, formed of bars, two millimetres thick and 25 millimetres broad, spaced 3 millimetres apart. The molten sulphur passing through the grate collects in the trough, and is removed from time to time by opening a valve.

In working the process the chloride of calcium solution, heated to 120 degrees, which is below its boiling-point, is run into the ore contained in one of the boilers, when the sulphur gradually melts out, collects, and is cast into moulds. In about two hours' time, when lump ores are treated, half the solution is run over into the second boiler, while the other half is drawn off into a vessel at a lower level, whence it is pumped up to the second boiler.

After washing out the exhausted residues with water to remove the adherent ley, the first boiler is refilled while the second is being extracted, the loss by evaporation from the solution being made up by the wash-water from the residue of the first operation. The residues from the lump ore retain only from four to five per cent. of sulphur, while the molten sulphur contains only 0·001–0·002 per cent. of foreign matters, while that of the calcaroni[1] process contains two to three per cent. of impurities, and only half the sulphur in the ore is recovered. With ores of an average yield of thirty-three per cent. the cost of working per ton is estimated at 12·75 francs. A further advantage is that the process may be worked all the year round, being entirely independent of atmospheric conditions, no sulphurous acid being found; while the calcaroni, on account of their deleterious influence upon vegetation, can only be worked during the first six months of the year.

H. B.

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[1] Calcaroni are kilns in which the liquation of the sulphur from the rock is effected by burning a portion of the ore.

F. Siemens' New Radiating Gas-Burner.

By Max Hermann.

(Journal für Gasbeleuchtung und Wasserversorgung, April, 1884, p. 237.)

By this burner, as with Siemens' regenerative burner, the illuminating power of the gas is increased by heating the air-supply, with the important difference however that, in the radiating burner, the heating is not effected by the escaping products of combustion, but by the radiated heat from the flame itself. The heat radiated from a flame increases with its illuminating power, and in order, at least partially, to utilize this important but unavoidable loss of heat, the lower part of the flame is surrounded by a reflector in such a manner that the greater part of the radiated heat is utilized for warming the air-supply. The heat thus utilized is greater than might be supposed. This is proved by the intensity of the flame obtained, it being possible to increase the illuminating power by 40 per cent., notwithstanding the loss of the direct light cut off from the portion of the flame surrounded by the reflector, and under certain circumstances this increase may be greater.

The burner consists of a hollow head or gas-chamber, upon which a number of small vertical tubes are fixed in a circle, these tubes being surrounded by a metal cylinder rising some distance above their upper ends. In the centre of the circle of tubes an upright rod is fixed, terminating in a knob or button, rising a short distance above the metal cylinder. The top edge of the cylinder is bent inwards, and a number of vertical ribs are formed on its inner side, similar ribs being fixed to the central rod. In the lower part of the cylinder, holes are made for the admission of the air-supply, and three projections are fixed on the outer side for supporting the glass cylinder, which latter must be of somewhat larger diameter than the metal cylinder to admit air for keeping the glass cool.

The gas for burning issues through the small vertical tubes, and receives its air-supply through the holes in the metal cylinder. As this air passes through the spaces between the small vertical tubes, it reaches the inner as well as the outer surface of the flame. The lower part of the flame is surrounded by the metal cylinder, at the top of which it is deflected inwards by the top edge of the cylinder being contracted, and it is again spread out by coming in contact with the knob on the top of the central rod. The portion of the flame within the metal cylinder is not visible, and it might therefore be supposed that the light obtained would be decreased: but this is not the case, as the upper part of the flame is so greatly increased in illuminating-power that there is an increase of from 30 to 40 per cent. over the ordinary Argand burner, due to the heating of the air-supply by contact with the metal cylinder and the central rod, and with the ribs formed on same.

With small burners the ribs cannot be used both on the cylinder and on the central rod, as the air space would be thereby too much contracted. The bending inward of the top edge of the cylinder, and the knob on top of the central rod, may also be omitted in small burners. For large burners, the heating surfaces of the ribs should be made as large as possible, and the cylinders may with advantage have the outer surfaces coated with some bad conducting material.

Although these radiating burners do not give results equal to regenerative burners, yet, compared with good Argand burners, they are worthy of notice, particularly on account of their simple and solid construction, and in consequence of the high temperature of the air-supply the combustion of the gas is so perfectly effected, that it is not so necessary to carry off the products of combustion as it is with ordinary burners.

The consumption and illuminating power of three sizes of burners made are given as follows:—

—	Consumption per Hour.	Illuminating Power.	Candles per Cubic Foot.
Size B . .	About 15·9 cub. ft.	65 Candles.	4·1 Candles.
„ C . .	11·3 „	50 „	4·4 „
„ D . .	7·0 „	32 „	4·5 „

C. G.

The Specific Heat of Water. By A. W. Velten.

(Wiedemann's Annalen, vol. xxi., part 1, 1884, p. 31.)

The experiments were made by the two well-known methods of mixture and the fusion of ice. The first method gives the ratio of the mean specific heat between two temperatures T and t to the mean specific heat between temperatures t and θ. The Author gives a long series of results in this form. In order to render these results comparable with those of other observers, it is necessary to refer them to some definite unit of heat. If as unit the value of the mechanical equivalent of heat at Baltimore is taken, the Author's experiments can be compared with Rowland's. Let W_t^T be the heat capacity between temperatures T and t, then the following Table shows how far the results agree.

These results have an important bearing on the question whether the specific heat of water increases or diminishes with the temperature. From the Author's experiments it appears that the mean specific heat between 4·7° and 7·5° is decidedly less than between 7·5° and 18·5°, and also less between 8° and 12° than between 12° and 24°. Hence it follows that there is an increase

—	By the Method of Mixture.	Observer.	By Rowland's Determination of the Mechanical Equivalent.	Difference.	Percentage Difference.
$\frac{W^{24}_{20}}{W^{29}_{24}}$	0·7986	Rowland	0·8036	−0·0050	−0·6
$\frac{W^{29}_{21}}{W^{36}_{29}}$	1·1376	,,	1·1423	−0·0047	−0·4
$\frac{W^{42}_{27}}{W^{27}_{20}}$	2·1553	Münchhausen	2·140	+0·015	+0·7
$\frac{W^{42}_{25}}{W^{25}_{17}}$	2·1412	,,	2·120	+0·021	+1·0
$\frac{W^{42}_{26}}{W^{26}_{17}}$	1·7915	,,	1·773	+0·018	+1·0
$\frac{W^{42}_{18}}{W^{18}_{9}}$	2·1519	Velten	2·170	−0·018	−0·9
$\frac{W^{42}_{15}}{W^{15}_{7}}$	3·3537	,,	3·359	−0·005	−0·2
$\frac{W^{42}_{23}}{W^{23}_{8}}$	1·2626	,,	1·261	+0·0013	+0·1
$\frac{W^{42}_{19}}{W^{19}_{5}}$	1·6335	,,	1·6333	+0·0002	0

from 4·7° up till 12° at least. Hirn observed a marked increase up to 12°, and Regnault from 5° to 25°, the line of increase of specific heat being approximately parallel to the line of increase of volume. Gerosa also found an increase up to 30°. On the other hand both Pfaundler and Platter and Rowland concluded that there was a diminution from 8°. For temperatures from 25° to 40° the observations both of Rowland and the Author show a diminution, whereas Münchhausen finds an increase. For still higher temperatures, the results of the several observers are given below.

Velten	$\frac{\text{Mean specific heat between } 97 \cdot 4^\circ \text{ and } 26 \cdot 8^\circ}{\text{Mean specific heat between } 26 \cdot 8^\circ \text{ and } 16 \cdot 56^\circ}$	=0·9924
,,	$\frac{\text{Between } 98 \cdot 3^\circ \text{ and } 27 \cdot 32^\circ}{\text{Between } 27 \cdot 32^\circ \text{ and } 18 \cdot 97^\circ}$	=0·9883
,,	$\frac{\text{Between } 99 \cdot 6^\circ \text{ and } 21 \cdot 96^\circ}{\text{Between } 21 \cdot 96^\circ \text{ and } 15 \cdot 64^\circ}$	=0·9941
Regnault	$\frac{\text{Between } 97 \cdot 6^\circ \text{ and } 14 \cdot 9^\circ}{\text{Between } 14 \cdot 9^\circ \text{ and } 8^\circ}$	=1·00709
,,	$\frac{\text{Between } 98 \cdot 1^\circ \text{ and } 16 \cdot 4^\circ}{\text{Between } 16 \cdot 4^\circ \text{ and } 8^\circ}$	=1·00890
Baumgartner	$\frac{\text{Between } 98 \cdot 16^\circ \text{ and } 14 \cdot 51^\circ}{\text{Between } 14 \cdot 51^\circ \text{ and } 1 \cdot 36^\circ}$	=1·0185
,,	$\frac{\text{Between } 98 \cdot 15^\circ \text{ and } 22 \cdot 30^\circ}{\text{Between } 22 \cdot 30^\circ \text{ and } 12 \cdot 10^\circ}$	=1·0115
Rowland	$\frac{\text{Between } 28^\circ \text{ and } 100^\circ}{\text{Between } 18 \cdot 5^\circ \text{ and } 28^\circ}$	=1·0024

From these to some extent contradictory observations the Author concludes that (1) from 5° to 12° there is an increase, and from 26° to 40° a decrease, and therefore a maximum between 12° and 26°. (2) From 40° to 70° there is an increase, and therefore a minimum at about 40°. (3) Above 100° there is a diminution, and therefore a maximum at about 100°.

The following empirical formula expresses these results fairly accurately:

$$k_t = 1 - 0\cdot0014625512\,t + 0\cdot0000237981\,t^2 - 0\cdot00000010716\,t^3$$

k_t being the specific heat at the temperature t.

The Author next proceeds to describe his experiments by the method of fusion of ice, and to compare the results with those of others. The following Table gives the mean specific heats (C) for various limits of temperature obtained by this method of experiment. The results are expressed in grams of mercury, *i.e.*, the weight of mercury occupying the volume equal to the contraction in volume, due to the melting of a portion of the ice of the calorimeter when a gram of water at the given temperature is introduced and allowed to cool 1°.

	°		°	Gram.		°		°	Gram.
C (from	0	to	7·31)	0·015869	from	7·31	to	10·87	0·015365
C (,,	0	,,	10·87)	0·015704	,,	10·87	,,	12·34	0·015461
C (,,	0	,,	12·34)	0·015675	,,	12·34	,,	14·59	0·015461
C (,,	0	,,	14·59)	0·015642	,,	12·34	,,	18·36	0·015556
C (,,	0	,,	18·36)	0·015636	,,	14·59	,,	18·36	0·015613
C (,,	0	,,	23·04)	0·015595	,,	18·36	,,	23·04	0·015434
						18·36	,,	27·67	0·015461
C (,,	0	,,	27·67)	0·015577	,,	23·04	,,	27·67	0·015487
C (,,	0	,,	40·58)	0·015486	,,	27·67	,,	40·58	0·015291
C (,,	0	,,	42·14)	0·015477	,,	40·58	,,	42·14	0·015243
C (,,	0	,,	56·13)	0·015445	,,	42·14	,,	56·13	0·015349
C (,,	0	,,	70·95)	0·015457	,,	56·13	,,	70·95	0·015503
C (,,	0	,,	99·68)	0·015471	,,	70·95	,,	99·68	0·015506

These results are in very close accordance with those previously recorded, and give an almost identical curve of relation between the specific heat and the temperature. In order to compare the results absolutely, the empirical formula given above may be used. From it

$$W_0^{99\cdot68} = 97\cdot6259 \qquad W_0^{27\cdot67} = 27\cdot2624,$$

and therefore

C (from 27·67° to 99·68°) = 0·977134 expressed in the unit of the formula.

Hence from the second Table,

0·977134 formula units = 0·015430 gram of mercury,

and therefore

The unit of the formula = 0·015791 gram of mercury.

Making use of this reduction factor to change the numbers given in the second Table to the same unit as that employed in the formula, and comparing the results with those obtained by the first method, the difference nowhere exceeds 0·2 per cent. which is a remarkable proof of the accuracy of the determinations.

E. H.

On the Diffusion of Gases through a Porous Wall.

By Gustav Hansemann.

(Wiedemann's Annalen, vol. xxi., part 4, 1884, p. 546.)

The experiments recorded in this Paper were undertaken with a view to proving experimentally Stefan's theory of the diffusion of gases through porous diaphragms. Stefan considered the motion of a gas through a diaphragm as a particular case of the general theory of the motion of masses of gas among each other, regarding the diaphragm as equivalent to a gas the particles of which were fixed. Many experiments had been previously made for the demonstration of the theory, particularly in relation to the proportionality between the velocity of diffusion and the difference of pressure on the two sides of the diaphragm, on the truth of which the theory depends. For small differences of pressure, the law had been verified; but no experiments had been undertaken, when the differences of pressure were considerable. By all previous experimenters Bunsen's diffusiometer had been employed in a more or less modified form. The difficulty of using this apparatus increases very much, as the differences in pressure become larger. This induced the Author to construct a new form of diffusiometer, in which the volume of gas remained constant, and the varying pressure was observed, means being provided at the same time for properly mixing the gas by agitation. The apparatus consisted of two chambers A and A^1, separated by a porous diaphragm of gypsum. Each chamber communicated with a system of receivers and pumps for regulating the supply and pressures of the gases. The pressures were measured by means of a steel plate or diaphragm stretched across apertures in the walls of each chamber. By a system of springs and levers the motion of the plate was communicated to a mirror, the deflection of which was read by a lamp and scale. In the chamber A a short spindle carried a fan or stirrer, which was rotated by a magnet external to the chamber, acting on a second magnet carried on the spindle of the fan. Both chambers were also provided with a manometer.

Kirchhoff[1] has shown analytically that the differential equations of Stefan can be integrated for the case of a single gas passing through a diaphragm originally supposed to be filled with the same gas at the same pressure, and has deduced the rate of diffusion of two different gases freely diffusing into each other from their coefficients of diffusion through a diaphragm. Taking the first term of Kirchhoff's integral—

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[1] The Paper of Kirchhoff is printed in the Annalen immediately after the Paper of the Author's.

$$k = \frac{\log(P_0'' - P_0') - \log(P'' - P') - 0\cdot 0004}{0\cdot 001545\, t},$$

when P′ and P″ are the pressures, and t the corresponding time. Making use of this expression, the Author found the following values of k for hydrogen diffusing into a vacuum, the temperature being 13·8° C.

P′.	P″.	P″ − P′.	k.
759·1	0·7	..	..
662·0	97·9	564·1	27·54
611·4	147·6	463·8	27·53
551·4	206·3	345·1	27·60
473·5	282·4	191·1	27·65
425·5	329·4	96·1	27·63
398·5	355·4	43·1	27·77
384·4	369·7	14·7	27·70
		Mean . .	27·63

For oxygen at a temperature between 13·4° and 13·6° C., the mean value of k was 8·628, and for a temperature of 13·7° k was 8·590. Both sets of experiments fully confirmed the law of proportionality.

Proceeding next to the interdiffusion of the two gases through the diaphragm, and taking the value of k as given by Kirchhoff's analysis, the Author finds for $k p_0$ with the above gases at a temperature of 13·7°, the following mean values:—

3314, 3289, 3196, 3003, 2812, 2734, 2634

for differences of pressure up to about 120 millimetres of mercury. Here the units of time and length are the minute and centimetre, and p_0 is the normal pressure of the atmosphere, 760 millimetres. These values exhibit very great deviations, and show that the theory of Stefan, based on the assumption that gases diffuse in the small interstices of a diaphragm with the same facility as in larger spaces, cannot be maintained. It is noteworthy that Loschmidt found by a direct method 36786 as the value of $k p_0$, more than ten times as great as that deduced by the Author from Stefan's theory. This would appear to show that the diaphragm has a very considerable retarding effect on the diffusion.

E. H.

The Dependence of the Coefficient of Conduction of Heat in Gases on the Temperature. By A. WINKELMANN.

(Wiedemann's Annalen, vol. xix., part 4, 1883, p. 649.)

In the present Paper the Author describes the continuation of his researches on the temperature coefficients of conduction in gases. His previous experiments were based on the assumption, supported by theoretical considerations, that the temperature coefficient must be the same for air and hydrogen. The results of the experiments on these two gases gave 0·00277 as the mean value of the temperature coefficient. Grätz has recently made fresh determinations, and obtained the values; for air, 0·00185, and for hydrogen 0·0016. These results were obtained by two different experimental methods, which apparently gave results in close agreement; but the Author maintains that Grätz has not correctly interpreted his observations, and that when rigidly worked out they lead to very different results. The Author has therefore undertaken a new series of observations.

If the identity of the temperature-coefficients for air and hydrogen be assumed, the readiest experimental method is to allow a thermometer to cool through a given range of temperature t in (1) a vessel containing air, (2) in a vessel containing hydrogen. Let V_{lt} and V_{wt} be the respective velocities of cooling. Then—

$$V_{lt} = l_t + s_t$$

$$V_{wt} = w_t + s_t,$$

where s_t is the term due to radiation, which is assumed to be independent of the medium.

Taking another temperature T, we have two similar equations—

$$V_{lT} = l_T + s_T$$

$$V_{wT} = w_T + s_T,$$

and hence, on the assumption of the equality of the temperature coefficients,

$$A = \frac{l_T}{l_t} = \frac{w_T}{w_t} = \frac{V_{wT} - V_{lT}}{V_{wt} - V_{lt}}.$$

An obvious objection to this method is that l_t and w_t are small compared with the terms due to radiation.

The second experimental method, which does not involve the assumption of the equality of the coefficients, is to allow a thermometer to cool over a given range of temperature in two different vessels of radii R_1 and R_2, then by making two sets of observations for the same temperature the terms due to the radiation and the conduction along the stem of the thermometer can be eliminated.

By a second series of observations with different temperature differences, A can be evaluated in the form—

$$A = \frac{x_T}{x_t} = \frac{C_T (V_1 - V_2)}{C_t (v_1 - v_2)},$$

where x_T is the conductivity of the gas for the temperature T, V and v the velocities of cooling at the temperatures T and t respectively, and C_T, C_t the heat capacity of the thermometers at these temperatures. In this method it is assumed that the radiation is independent of the dimensions of the enclosing vessel. It may be noted that the radiation term can be reduced, and therefore the determination made more accurate, by using a silvered thermometer. The Author in his experiments on air has made use of this second method.

After describing more particularly the construction of the apparatus, the results for air are first given. Using unsilvered thermometers in two different sets of apparatus, the values of the temperature coefficients were found as 0·00227 and 0·00128. Of these two values the former has the greater probability of correctness, as it is least affected by experimental errors. Using a silvered thermometer, the results were 0·00189, 0·00170, and 0·00148. For the determination of the coefficient for hydrogen, the first method had necessarily to be employed, as the rate of cooling in the smaller vessel was too great to be determined accurately. The results obtained were 0·00162 and 0·00174 with unsilvered thermometers, and 0·00138, 0·00184, and 0·00137 with the silvered thermometers. Another arrangement of the apparatus gave the values 0·00243, 0·00250, and 0·00236. Arranging these observations into two groups according to the apparatus used, and taking the mean we have for air and hydrogen, 0·00166 and 0·00243 respectively. These are subject to an important correction for the change in the capacity of the thermometers with increasing temperature, due to the change in the specific heat of the glass and mercury. When this correcion is made, the above mean values become 0·00173 and 0·00231, assuming that the specific heat of the glass only changes, or 0·00160 and 0·00208 assuming a change of specific heat both of the glass and mercury.

The Author next considers, at considerable length, the effect of the conduction along the stem of the thermometer. He concludes that the value 0·00208 is the nearest approximation to the truth. According to the theory of Clausius, the temperature-coefficient for air and hydrogen must be 0·00183, which is in fair agreement with the above result. On the other hand it should be noted that this result is not in accord with the observed temperature coefficient of viscosity, which, for gases whose specific heat is independent of the temperature, should be the same as the temperature coefficient of conductivity. Meyer, Puluj, Warburg, E. Wiedemann, and v. Obermayer, all agree that the former is not far different from 0·0027.

Proceeding in the same manner with carbonic acid gas by the second method of experiment, and with a silvered thermometer, the Author finds 0·00406 and 0·00380 as the nearest approximations to the value of the temperature-coefficient, according as the specific heat of mercury is taken as constant or diminishing with the temperature. The effect of absorption in the gas would probably make this value a little too small.

E. H.

On the Heat developed in Iron by a Periodic Alternating Magnetic Force. By E. Warburg and L. Hönig.

(Exner's Repertorium der Physik, vol. xix., 1883, p. 741.)

Assume that the changes in magnetising force are produced by the motion of a permanent magnet, and that there are no other conductors in the field except the iron experimented on; then if the magnetising force pass through a cycle of values, the heat developed in the iron is equivalent to the work done in the cycle. The work A can then be simply expressed in terms of m, the component of the magnetic moment in the direction of k the magnetising force;

$$A = -\int m\,d\,k,$$

the integration extending over the cycle of values of k. If the magnetic moment m had always the same value for a given value of k, then the integral over the limits of the cycle would be zero; but it is known that m is greater for a given value of k, when k is diminishing than for the same value, when k is increasing. Hence, if the corresponding values of k and m be represented graphically, A will be the area of a closed curve.

There are three causes known, which to some extent explain this phenomena. (1.) If k changes indefinitely slowly, the so-called "coercive force" accounts for some of the heat developed. This can be measured, as the first-named Author has previously shown, by statical experiments. It may be called, for convenience, "the magnetic friction heat." It is not known, if the coercive force has any effect, when the changes in k take place with finite velocity. (2.) The iron being a conductor, currents are induced in its mass, which tend to increase the magnetic moment when k is diminishing, and to diminish it when k is increasing. The heat developed by these currents may be called the "electro-magnetic heat." This need only be considered when the changes take place with finite velocity. (3.) It is known that the coefficient of magnetic induction is a function of the temperature of the iron,[1] hence the integral $\int m\,d\,k$ will vanish only when the values of t and k are connected by a relation which can be expressed by $f(t, k) = 0$ throughout the cycle, or in the case of an adiabatic cycle.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxi., p. 517; and vol. lxxii., p. 417, and vol. lxxv., p. 421.

The Authors proceed to describe the experiments made to determine the part of the heat developed due to each of these causes, and to compare the determinations of the magnetic friction heat with those made by statical methods, when the velocity of change was indefinitely small. The heat was measured directly by an ether calorimeter, and as the quantity to be measured was very small, the apparatus required to be exceedingly delicate, and the observations to be made with great care. The Authors tried two iron bars, and three bundles of iron wire, each made up of wire of different diameters, as subjoined:—

	Centimetre.
Bundle I.	0·011
Bundle II.	0·034
Bar I.	0·4
Bar II.	0·7

Bundle III. was made up of strips of sheet iron, 0·7 centimetres broad and 0·0165 centimetre thick. The magnetising force employed was about 170 times as great as the horizontal component of the earth's magnetism at Freiburg. The observations were made in each case (1) when the cycle of k extended from 0 to $+k$; and (2), when the cycle was from $-k$ to $+k$. The suffixes in the following Table distinguish between the simple and double cycle. N is the number of wires in the bundle, M the mass, F the magnetic friction heat determined statically, and W the heat measured by the calorimeter.

——	N.	M.	F_1.	W_1.	F_2.	W_2.	$\frac{F_2}{F_1}$.	$\frac{W_2}{W_1}$.	$\frac{W_1}{F_1}$.	$\frac{W_2}{F_2}$.
		Grams.								
Bundle I.	307	14·77	9·2	5·3	28·0	17·6	3·04	3·32	0·58	0·63
Bundle II.	150	14·47	4·9	5·2	17·6	17·4	3·59	3·35	1·06	0·99
Bar I.	1	13·01	5·1	13·3	18·9	46·0	3·71	3·46	2·61	2·43
Bar II.	1	39·32	0·40	5·6	1·60	10·1	4·00	1·80	14·0	6·31
Bundle III.	..	13·00	3·7	2·4	12·0	7·8	3·24	3·25	0·65	0·65

The maximum value of k was not far removed from the point where the curve of magnetization breaks away from the straight line. The coefficient of magnetic induction for the maximum value of k was as follows for the several bundles:—

Bundle I.	21·7
Bundle II.	20·8
Bundle III.	20·1
Bar I.	12·9
Bar II.	7·2

If the coercive force were null, and the heat in the calorimeter experiments due only to the electrical conductivity of the iron, $\frac{W_2}{W_1}$ must equal 2. With the exception of bar II., the reason for which is not clear, $\frac{W_2}{W_1} > 2$.

The part of W due to the effect of temperature on the magnetic moment can be approximately calculated. Let $d\,w$ be the heat, which must be communicated to a mass of iron, of volume v and density d, when the magnetising force is increased by $d\,k$, and the temperature $d\,\mathrm{T}$. Then

$$d\,w = \mathrm{M}\,d\,k + \mathrm{N}\,d\,\mathrm{T} \quad . \quad . \quad . \quad . \quad (1)$$

and also by Carnot's principle,

$$\mathrm{M} = \frac{\mathrm{T}}{\mathrm{J}}\frac{d\,m}{d\,\mathrm{T}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where J is Joule's equivalent in absolute measure. If the change is adiabatic,

$$d\,\mathrm{T} = -\frac{\mathrm{M}}{\mathrm{N}}\,d\,k \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

As a first approximation N may be considered independent of T and k, and we may put

$$\mathrm{N} = c \cdot v \cdot \sigma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where c is the specific heat of the iron. Also, may be put

$$m = \mu\,(1 + \beta\,t) \cdot k \cdot v \quad . \quad . \quad . \quad (5)$$

and therefore,

$$\frac{d\,m}{d\,T} = \mu \cdot \beta \cdot k \cdot v \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and $$\mathrm{M} = \frac{\mathrm{T}}{\mathrm{J}} \cdot u \cdot \beta \cdot k\,v. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Let the constant $\frac{\mu\,\beta}{2 \cdot \mathrm{J} \cdot c \cdot \sigma}$ be represented by f; then, when the change is adiabatic

$$\mathrm{T} = \mathrm{T}_0 \cdot e^{-f\,(k^2 - k_0{}^2)} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or if $k_0 = 0$ $$\mathrm{T} = \mathrm{T}_0 \cdot e^{-f\,k^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and since $f\,k^2$ is very small, there may be written

$$\mathrm{T} - \mathrm{T}_0 = -\,\mathrm{T}_0 \cdot f \cdot k^2 \quad . \quad . \quad . \quad (10)$$

This equation gives the greatest change of temperature θ, which can take place if we put for k its maximum value. The difference of temperature throughout the cycle can then not exceed 2θ, and if y be the difference of magnetic moment for a mass of 1 gram,

$$y < \frac{\mu \cdot k}{\sigma} \cdot 2\beta \cdot \theta.$$

The area A_1 of the space enclosed by the curve of the cycle is then $< \int y\,dk$, and therefore $< \frac{\mu \cdot \theta \cdot k^2 \cdot \beta}{\sigma}$. According to Wiedemann, the temporary magnetic moment of an iron bar diminishes 0·5 per cent., as the temperature increases from 20° to 100°; therefore, $\beta = -\frac{1}{10}^4$, also $\mu = 20$, $c = 0{\cdot}114$, $\sigma = 7{\cdot}7$, and $J = 42500{\cdot}981$, and hence $\theta = 8{\cdot}6 \times 10^{-6}$, and $A_1 < 2{\cdot}6 \times 10^{-6}$. This calculation shows that the heat due to this cause is negligible compared with that due to the first cause.

A superior limit may also be deduced by calculation for the electro-magnetic heat. Let E represent the electro-magnetic heat, then the Authors calculate the following values as limits:—

——	E_1.	W_1.	E_2.	W_2.	$\frac{E_1}{W_1}$.	$\frac{E_2}{W_2}$.
Bundle I. . . .	0·58	5·3	1·76	17·6	0·11	0·10
Bundle II. . . .	1·34	5·2	3·22	17·4	0·26	0·19
Bar I.	73·3	13·3	183·3	46·0	5·5	4·0
Bar II.	78·0	5·6	140·4	10·1	13·9	13·9

Comparing this Table with the previous ones, the Authors conclude—

(i.) That the increase in the ratio $\frac{W}{F}$, as the diameter of the wire is increased, is explained by the increase in $\frac{E}{W}$, *i.e.*, is due to the electro-magnetic heat.

(ii.) The electro-magnetic heat is less than 10 and 11 per cent. of the total heat in the case of bundle I., and less than 26 and 19 per cent. in the case of bundle II. for the simple and double cycle respectively.

(iii.) The actually observed heat W is, in the case of bundle I., about two-thirds of the magnetic friction heat when the cycle is performed with indefinitely small velocity, and the same conclusion holds for the bundle made up of strips. It hence appears that the work done against the so-called coercive force is very different according as the change in the magnetising force takes place in a fraction of a second or indefinitely slowly.

E. H.

On the Spontaneous Oxidation of Mercury. By D. Macaluso.

(Exner's Repertorium der Physik, vol. xix., 1883, p. 801.)

Berthelot has shown that the skin which forms over the surface of mercury in contact with air is really oxide of mercury. The Author's experiments were undertaken to determine what effect moisture had on the formation of this skin. The apparatus consisted of a glass barometer tube with an extended bulb at the upper end, communicating with a Bessel-Hagen pump and also with a mercury reservoir. The barometer was filled by distilling mercury over from the reservoir, and then sealing the tube communicating between them. Dry air, moist air, and pure aqueous vapour were alternately introduced into the vacuum above the mercury column. Due care was taken to have the glass properly clean and dry before introducing the mercury, which had been previously distilled in a vacuum before being used in the apparatus. It was found that—

(*a.*) In the tube filled with moist air there was rapid oxidation of the mercury. Even after only twenty-four hours' standing brown streaks were visible on the glass where it touched the mercury, if the containing vessel was slightly agitated, and after some days the same appeared on the free surface of the mercury.

(*b.*) In the tube filled with dry air not the least change was observed, even after two months had elapsed from the time of filling the tube. Agitation left no streaks on the glass, and no skin was visible when a glass stirrer was introduced.

(*c.*) The same was the case when the tube was filled with pure aqueous vapour not saturated.

It thus appears that mercury, like many other metals, does not undergo oxidation either in dry air or in aqueous vapour, but only when aqueous vapour is present in the air. This points to an electrolytic explanation of the oxidation.

E. H.

On the Influence of Temperature on the Electromotive Force of a Galvanic Element, and its effect on the Free Energy of the Same.

By Siegfried Czapski.

(Wiedemann's Annalen, vol. xxi., part 2, 1884, p. 209.)

Helmholtz and Sir William Thomson have demonstrated that the electromotive force of a galvanic element is proportional to the electrothermic energy of the chemical processes due to the passage of the current, assuming the truth of Joule's law on the heating of conductors by a current passing through them, of the principle of conservation of energy, and of the generally accepted electrolytic actions in the element. Bosscha has shown that certain experiments of Favre are not in accord with this deduction, and has concluded that Joule's law cannot be extended to the heating of an electrolyte. More recently Braun from a large number of experiments has come to the conclusion, in contradiction to the accepted theory, that certain cells have a greater electromotive force than one would expect from the changes of chemical energy, and on the other hand, that in other cells the electromotive force is less. Alder Wright has also come to a similar conclusion. The Author has continued the researches of Helmholtz on the effect of temperature.

Let E be the electromotive force of an element, q_1 q_2 the heat equivalents in mechanical units of the positive and negative chemical actions taking place in the element, *i.e.*, the heat produced by the chemical action due to the passage of unit quantity of electricity, then Braun has shown that—

$$E = x\, q_1 - y\, q_2.$$

When x and y are certain constants depending on the nature of the cell, and that only in particular cases is $E = q_1 - q_1$. As Helmholtz has proved, the action in a galvanic cell, when the intensity of the current is indefinitely small, is completely reversible, and hence the second law of thermo-dynamics applies. This follows from the consideration that the heating effect due to resistance, according to Joule's law, varies as the square of the current, and therefore becomes of the second order of small quantities. Let a quantity of electricity, e, have passed through an element at a given time, and let θ be the absolute temperature, then the energy π is a function of e and θ. Let p be the electromotive force of the element, and let a quantity of heat $d\,Q$ be communicated to it during the process, then—

$$d\,Q = \frac{d\,\pi}{d\,\theta}\,d\,\theta + \left(\frac{d\,\pi}{d\,e} + p\right)d\,e,$$

according to the principle of the conservation of energy. Also by the second law of thermo-dynamics, if S be the entropy of the system—

$$\begin{aligned} d\,S &= \frac{1}{\theta}\cdot d\,Q \\ &= \frac{1}{\theta}\frac{d\,\pi}{d\,\theta}\,d\,\theta + \frac{1}{\theta}\left(d\pi \quad + p\right)d\,e, \end{aligned}$$

and also—
$$= \frac{dS}{d\,\theta}\,d\,\theta + \frac{d\,S}{d\,e}\,d\,e.$$

Hence—
$$\frac{dS}{d\,\theta} = \frac{1}{\theta}\,\frac{d\,\pi}{d\,\theta} \text{ and } \frac{d\,S}{d\,e} = \frac{1}{\theta}\left(\frac{d\,\pi}{d\,e} + p\right),$$

whence by differentiation—

$$\frac{d\,\pi}{d\,e} + p = \theta\,\frac{d\,p}{d\,\theta}.$$

Substituting in the original equation this gives,

$$d\,Q = \frac{d\,\pi}{d\,\theta}\,d\,\theta + \theta\,\frac{d\,p}{d\,\theta}\,d\,e.$$

It hence appears that $\theta\,\frac{d\,p}{d\,\theta}\,d\,e$ is the quantity of heat necessary to keep the element at constant temperature, when the quantity of electricity $d\,e$ passes through it. It follows that those cells which do not convert all the energy due to the chemical action into electrical energy have an electromotive force, which diminishes as the temperature increases, and conversely those cells which work in part at the cost of their own heat have an electromotive force which increases with the temperature.

If the temperature θ be maintained constant, the foregoing equation becomes—

$$d\,Q = \theta\,\frac{d\,p}{d\,\theta}\cdot d\,e,$$

and its integral is—

$$Q = \theta\cdot\frac{d\,p}{d\,\theta}\cdot e.$$

The experimental portion of the research consists, therefore, in the determination of $\frac{d\,p}{d\,\theta}$ and e. Some experiments have recently been made by Preece on the value of the former for various cells. (Proceedings of the R. S. 35, p. 48, 1883.)

The Author used as his standard of electromotive force, to which all measurements were referred, a cell of zinc and mercury in solutions of chloride of zinc and calomel. Two of these cells were made up, and protected from oxidation by enclosing them in vessels from which the air was exhausted. Their temperatures were maintained constant by means of a water bath. The electromotive force was found to be 1·0884 and 1·0904 volt respectively, assuming Weber's determination of his normal Daniell, viz., 1·0954 volt. This value depends upon his determination of the ohm, which is not in agreement with the results of other physicists. The cells, for which the value of $\frac{d\,p}{d\,\theta}$ was to be determined, could only be chosen from a limited number of types, as it was essential that they should be reversible, and their chemical actions well defined. This is the case with cells consisting of

Metal A | Salt of metal A | Salt of metal B | Metal B ;

such, for example as the standard cell above described. It is further essential that the one salt should be insoluble in the other, and they should each be neutral towards their respective metals, and also that the metals should be pure, and in particular each free from impurities containing the other. Such an element must obviously be reversible.

The cell first experimented with was the chloride of silver element of Warren de la Rue. Representing by K_1 K_2 the electromotive forces of the two standards, the mean of two series of observations gave—

$$p = 0{\cdot}9057\ K_1 \text{ at } 16^\circ \text{ Centrigrade,}$$

$$\frac{dp}{d\theta} = -\,0{\cdot}00024\ K_1,$$

$$\theta \frac{dp}{d\theta} \frac{e}{J} = -\,3{\cdot}39 \text{ kilogram-degrees Centigrade.}$$

This should be equal to the difference between the work done by the current, when the circuit was closed, and the energy resulting from the chemical actions during the time of the experiment determined independently. The former was 44·32 kilogram-degrees, and the latter, though not accurately known, can be shown to be less than 54·08, and considerably greater than 38·45 kilogram-degrees. Hence the difference is greater than 9·76 kilogram-degrees.

The same element, with a much stronger solution of chloride of zinc gave—

$$p = 0{\cdot}8108\ K_1 \text{ at } 17^\circ \text{ Centigrade,}$$

$$\frac{dp}{d\theta} = -\,0{\cdot}000195\ K_1,$$

and $$\theta \frac{dp}{d\theta} \frac{e}{J} = -\,2{\cdot}77 \text{ kilogram-degrees.}$$

Calculating as before the difference between the energy of the chemical actions and the work done by the current, it is found to be greater than $-\,15{\cdot}60$, and less than 1·22 kilogram-degree. A third element, made of chemically pure zinc, very dilute chloride of zinc solution, and chloride of silver in contact with a silver wire, the whole being enclosed in an exhausted vessel, gave very regular results. The electromotive force between 19° and 31° Centigrade was approximately a linear function of the temperature, and—

$$p = 0{\cdot}91972\ K_1 \text{ at } 19^\circ \text{ Centigrade,}$$

$$\frac{dp}{d\theta} = -\,0{\cdot}0003606\ K_1,$$

$$\theta \frac{dp}{d\theta} \frac{e}{J} = -\,5{\cdot}12.$$

Work done by the current	$=$	44·69	kilogram-degrees.	
Energy of chemical actions	$<$	54·08	,,	,,
and hence difference	$>$	$-\,9{\cdot}39$	,,	,,

The fourth element tried consisted of iron wire in a solution of ferrous chloride and calomel and mercury. The electromotive force was found to be best expressed by a quadratic function of the temperature.

$$p = (0\cdot65681 + 0\cdot001366\,\theta - 0\cdot0000245\,\theta^2)\,\mathrm{K}_1,$$

and hence at 20° Centigrade—

$$p = 0\cdot67433\,\mathrm{K}_1,$$

$$\frac{dp}{d\theta} = 0\cdot000337\,\mathrm{K}_1,$$

and

$$\theta\frac{dp}{d\theta}\cdot\frac{e}{\mathrm{J}} = 4\cdot85 \text{ kilogram-degrees.}$$

Also work done by the current	= 33·00	„	„
Energy of chemical actions	< 17·40	„	„
and hence difference about	= 15·6	„	„

In this case, it will be seen, that the electromotive force rises with the temperature to a certain point, and then diminishes as the temperature further increases. It is found, on the other hand, that the energy of the chemical action does not vary much with the temperature, and hence $\frac{d^2 p}{d\,\theta^2} = 0$, which is not in accord with the former result. The Author found that the linear expression—

$$p - (0\cdot65681 + 0\cdot000663\,\theta)\,\mathrm{K}_1$$

at 20° Centigrade, very fairly represented the results.

The Author further gives the results of similar experiments with an element like the last-mentioned, but with a more concentrated solution of ferrous chloride, and also with elements similar to the normal cells described above, with bromide substituted for chlorine, and also with cadmium in the place of zinc.

E. H.

On the Electromotive Force of the Daniell Battery.

By Emile Reynier.

(L'Electricien, No. 70, March 1884, p. 201.)

The Author records the results of a series of experiments to determine the electromotive force of a Daniell's element, made up in a number of different ways. The liquids used were the following: in the copper compartment (1) saturated sulphate of copper; (2) solution of sulphate of copper, acidulated by the addition of 5 per cent. by volume of sulphuric acid ($H_2SO_4 + H_2O$); in the zinc compartment (1) solution of sulphate of zinc, 500 parts by weight of salt with 1,000 parts of water; (2) solution of sulphate of zinc, acidulated with 5 per cent. of sulphuric acid; (3) solution of chloride of sodium, 200 parts of salt with 1,000 parts of water; (4) solution of chloride of sodium, acidulated with 5 per cent. of sulphuric acid; (5) water acidulated with 5 per cent. by volume of sulphuric acid. Experiments were also made with zinc cloisonné, the electrode with its covering of parchment paper being simply plunged into the copper liquid. The measurements were made a short time after the elements were mounted. The following Table gives the results, the first cell being the standard combination of the Post Office.

Liquids.		Electromotive Force.	
Copper-Compartment.	Zinc-Compartment.	Amalgamated Zinc.	Ordinary Zinc.
Solution of—	Solution of—	Volts.	Volts.
Saturated sulphate of copper	Sulphate of zinc . . .	1·079	1·068
" "	" acidulated	1·105	1·06
Acidulated "	" . . .	1·03	1·025
" "	" acidulated	1·066	1·03
Saturated "	Chloride of sodium . . .	1·145	1·14
Acidulated "	" . . .	1·115	1·09
" "	" acidulated	1·125	1·09
Saturated "	Acidulated water . . .	1·134	1·05
Acidulated "	" . . .	1·119	1·027
Saturated "	Zinc cloisonné	1·10	1·04
Acidulated "	"	1·05	1·00

E. H.

On the Results of the Electric-Light Installation in the Leipzigerstrasse and the Potsdamer Platze in Berlin.

By Fr. von Hefner-Alteneck.

(Electrotechnische Zeitschrift, February 1884, p. 60.)

The Author described in a previous Paper the engineering details of the installation. The present deals particularly with the commercial results, which are especially interesting as the circumstances were such as to allow a fair comparison with gas. The installation consisted of thirty-six arc-lamps, and the average length of time each arc was in use during the year was 1,900 hours, giving a total of 68,400 lamp-hours. Gas-engines were used entirely for motive-power, the gas supplied being of the same quality as that used simultaneously for lighting the neighbouring streets. The price of the gas was taken as 13·3 pfennige per cubic metre (approximately 3*s.* 9*d.* per 1,000 cubic feet), which is the estimated cost of the gas used for street lighting by the municipality of Berlin, and somewhat less than the retail price. The following is an analysis of the cost of maintenance from the 20th of September, 1882, to the 20th of September, 1883:—

	Marks.
Gas consumed (including the lighting of the machine-house)	10,156
Water	1,654
Oil and waste	2,096
Carbons for the arc-lamps	5,472
Repairs	540
Labour and attendance	3,836
Rent of ground for the machine-house	300
Fire insurance	91
Sundry disbursements	390
	24,535

The Author points out that as each arc-light required about 1 HP., the cost of the motive-power per HP.-hour is about 20 pfennige, or 2·4*d*. With a steam-engine in place of the gas-engines this might be reduced to 0·6*d*. The actual cost to the municipality of lighting an equal length of the neighbouring streets by gas, though not with the same illumination, was 26,000 marks.

The Author proceeds to discuss the question of the depreciation of the plant, and makes, in passing, the interesting observation that the cable employed, which consisted of a copper wire served with steeped hemp and sheathed with lead, showed no signs of deterioration, although subjected to an electromotive force of 650 volts.

E. H.

On the Electrolysis of Glass. By E. Warburg.

(Wiedemann's Annalen, vol. xxi., part 4, 1884, p. 622.)

Buff first determined the dependence of the conductivity of glass on the temperature. Later determinations have been made by Beetz, Perry, Foussereau,[1] and Gray. Buff and Beetz have further proved that galvanic polarization takes place, and Buff and Sir William Thomson have constructed galvanic elements in which glass plays the part of a conductor.

The Author describes his own experiments on this subject, and at the end of the Paper gives a summary of his results. In the electrolysis between mercury electrodes of a soda-lime glass, heated to a temperature of 300° Centigrade, a badly-conducting layer of silicic acid separates on the anode side of the glass, and reduces the current in a short time to a small part of its original value. This layer has a greater or less thickness, according as the electromotive force of the circuit for a given time has been greater or less. It exhibits the colours of thin plates, by which it can be readily recognised, and when cold reduces the superficial conductivity of the glass to a small part of its original value. When hot, if the mass of glass is conductive, it acts like the dielectric of a condenser, whose plates are represented by the mercury anodes. The capacity of this condenser is independent of the differences of potential between the plates, for variations between 1 and 20 Bunsen elements, and was found to be 0·021 and 0·041 microfarad per square centimetre of glass, according as the layer had been formed by 30 or 15 Bunsen elements, working for several hours. The formation of this layer, and the consequent weakening of the intensity of the current, can be avoided by using a sodium amalgam as anode. The result of the electrolysis can in this case be quantitatively established. The quantity of sodium abstracted from the kathode is deposited on the anode, so that the weight of the glass remains unaltered. Moreover, the sodium transferred is chemically equivalent to the quantity of silver deposited in the same time by the same current in a silver voltameter. The glass remains perfectly clear throughout the electrolysis, and its characteristic qualities are not markedly altered.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxv., p. 420.

E. H.

On the Electrical Phenomena of Flames. By J. KOLLERT.

(Wiedemann's Annalen, vol. xxi., part ii., 1884, p. 244.)

The Author has continued the researches of Hankel on this subject, and also discusses the results recently obtained by Elster and Geitel.[1] He concludes his Paper with a summary of the principal results of his investigation.

The electrification depends on the chemical constitution and the temperature of the muzzle of the burner. Iron in particular appears very strongly negative.

The electromotive force is a function of the position in which the electrode is placed in respect to the flame. The point of greatest electrification coincides with the point of maximum temperature. It is difficult to prove this directly by inserting a platinum wire into different portions of the flame, on account of thermo-electric effects, with which it may be easily confused. Independent observations, however, help to make the point clear. St. Claire-Deville has shown, that in the carbonic oxide flame the quantity of free carbonic oxide increases as the temperature rises, or otherwise, that the maximum dissociation corresponds to the maximum temperature. Applying these results to the carburetted hydrogen flame, the greatest quantities of free hydrogen and carbonic oxide will occur in the hottest parts. Further, Grove has shown that metals which do not decompose water are very strongly negative in contact with hydrogen and carbonic oxide. These observations only apply to cold metals and gases; but they may be extended to higher temperatures. Hence at the points of highest temperature in a flame, in consequence of the positive electrification of the platinum, due to the rise of temperature in the wire connecting the molecule of platinum with the burner, positive electricity passes from the burner to the platinum; and secondly, the introduction of the platinum into the flame, consisting of carbonic oxide and hydrogen, produces a displacement of negative electricity in the opposite direction, *i.e.*, the two effects are added. In the positions of lower temperature, occupied according to St. Claire-Deville by carbonic acid and aqueous vapour, which to a slight extent positively excite the platinum, the second effect is masked, and the thermo-electric effect is alone apparent. This also is the case for positions above and at the side of the flame, where the thermo-electric effect is much weaker. It must, however, be remembered, that flames have no homogeneous character, and the results must be taken as the mean of a number of observations.

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[1] Minutes of Proceedings Inst. C.E., vol. lxix., p. 499.

In conclusion, the Author points out that his results are in close accord with those of Elster and Geitel. They found for the flame of a Bunsen burner, that the electromotive force between a platinum wire outside the flame, and a second in the same transverse plane, but introduced as far as the centre of the flame, was equal to 1·9 that of a Daniell's cell; and for an alcohol flame 1·44 of a Daniell. The Author's results are respectively 1·95 and 1·43.

E. H.

Schultz's System of Artillery.[1]

(Note sur le Système d'Artillerie du Capitaine Schultz, May 1883.)

The principle of the Schultz system of artillery consists in providing separate and distinct organs for resisting the two stresses exerted by the gases of explosion—one parallel to the axis of the tube, the other radially at right angles to it—avoiding the concentration of the two stresses on one and the same piece, as in ordinary guns. To resist the radial stress, the tube of the gun is fortified by winding steel wires around the tube, under a given tensional stress, which need not be considerable, imposing on the tube a compressive stress greater than that of the opposing explosion of powder. The wires so applied vary from 1·03 to 2·5 millimetres, or from 0·04 inch to 0·10 inch in diameter. Since the tube is already compressed under a greater degree of stress than that due to the explosion, it will not be enlarged even to the natural volume it occupied previous to compression; and, since the material is practically not stretched at all by the explosion, the calibre remains unaltered, and the guns are, as Mr. Schultz says, "invariable." The number of strata of wire wound on the cannon, varies from 21 to 43, for guns respectively of 3½ inches to 13½ inches bore.

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[1] The original is in the library of the Inst. C.E.

In ordinary guns, the longitudinal stress is transmitted by the breech-screw to the tube which already resists the radial stress. Capt. Schultz carries the screw by a ring screwed into a steel collar or sleeve which covers and protects the wires, and takes a bearing on the back end of the tube, as well as on a shoulder formed on the tube beyond the wirework. This sleeve also carries the trunnions, to which it transmits the longitudinal pressure on the screw, without having in the slightest degree to resist the expansion of the tube. For the larger guns, the sleeve is, for the sake of lightness, replaced by longitudinal bolts and nuts.

A test-tube of 22 millimetres, or 0·866 inch bore, and 50 inches long, was constructed in 1871 by Capt. Schultz. The wire covering consisted of twelve layers of wires, 0·0276 inch, or $\frac{1}{36}$ inch in diameter, making up in all a thickness of 0·28 inch. The estimated total normal explosive tension on the tube was 54,600 lbs. per square inch. It was closed at each end by a plug screwed into a strong plate applied at each end, the two plates being bound together by four bolts. The first series of tests were made with increasing charges of sporting-powder, and forty charges, with the tube completely filled with powder, generating an absolute pressure of 6000 or 7000 atmospheres. At each discharge, the tube remained subject to the action of the gases for half an hour, after which the bolts were slackened. There was absolutely no variation in the diameters of the tube.

The tube was afterwards tested by Mr. Sarrau, who charged it with gun-cotton, completely filling the tube with it, at the density 0·541. The tube resisted pressures which had not before been realised, and burst with the pressure of 12,000 atmospheres, as measured by the Crusher gauge.

Two guns of 90 millimetres, or 3·54 inches bore, specially constructed for the Artillery Committee of the War Department in France, were tested in 1875 and 1876. No. 1 was loaded with shells of 21 lbs. and 24 lbs., and fired four hundred and thirty-three times, with charges of 5½ lbs., 6 lbs., and 6½ lbs. of powder. Finally, the gun was fired thirty-nine times with projectiles of 31 lbs., attaining a speed of 1509 feet per second. Compare these results with the work of the ordinary 90-millimetre gun, which only takes a projectile of 17½ lbs., with a charge of 4·20 lbs. of powder, attaining a velocity of 1490 feet per second. It is reported that the gun did not show any trace of fatigue after the test above noticed.

No. 2 gun weighed 1,150 lbs., against 1,875 lbs., the weight of No. 1. Only twenty-nine rounds were fired with this gun, as the carriage became disabled.

Two guns of 19 centimetres, or 7·48 inches, bore were next constructed for the Artillery Committee. The first of these was of cast-iron, not tubed. It was proved as follows:—

								Lbs.
At	Bourges,	20	rounds,	of which one was with a charge of				37½
„	Calais,	8	„	with a low charge and projectile of				166
„	„	410	„	with a charge of	35	lbs., and	ditto	166
„	„	50	„	„	35	„	„	218
„	„	50	„	„	37½	„	„	218
„	„	31	„	„	39½	„	„	218

At the thirty-first round of the last series the gun burst, owing to the breakage of two of the projectiles within the gun, by which it was fissured and became weakened. Nevertheless, the breech and its fittings remained in good condition. The Committee reported that the system of the division of resistances acted perfectly during the heavy tests above recited; and they noted that whilst the proof of the ordinary gun of 19 centimetres consists of twenty-five rounds, with 37½ lbs. of powder, with a projectile of 166 lbs., plus two charges of 39 lbs. of powder, giving a pressure of 32,200 lbs. per square inch, the Schultz gun made

50 rounds,	with 37½ lbs. of powder,	218-lb. projectile,	33,922 lbs. pressure.
31 „	39½ „	218 „	36,960 „

A steel gun of 19 centimetres, or 7·48 inches, bore was tested in July 1882, till it burst, with projectiles of 218 lbs. weight, and charges up to 49½ lbs. of powder. At the thirty-second round the breech-plate broke across through holes which had been drilled too near the bolt-holes. The tests were abandoned.

The Inspector-General of Artillery ordered and tested a steel gun of 10 centimetres, or 3·94 inches, bore. With a charge of 14¾ lbs. of powder, and a projectile of 33 lbs., a velocity of 1750 feet per second was attained, the corresponding velocity for the ordinary projectile of 22 lbs. for the same calibre being 2,290 feet per second. The pressure was 4,756 lbs. per square inch. After 324 rounds, the gun remained in good condition, excepting a flaw in the breech-ring.

It is concluded that the wire system of gun has the following advantages:—The limitless resistance of the tube to expansion, admitting of the use of very heavy projectiles, with strong charges of powder, giving velocities hitherto unreached, the invariability of the calibre, facility of manufacture, reduction of weight, and economy.

D. K. C.

The 16-*centimetre Gun of the Spanish Navy.*

By S. C. H. L. Dupuy de Lôme.

(Comptes rendus de l'Académie des Sciences, June 9, 1884.)

A 16-centimetre or 6·3 inch gun has been constructed by the Société des Forges et Chantiers, for the Spanish Government, under the direction of Mr. Canet, Assoc. M. Inst. C.E., to the plans of

General Hontoria. In the design of ordinary guns, hitherto, the chief object has been to provide power of resistance to bursting or splitting, without engaging the superposed rings to resist the unbreeching of the gun by longitudinal stress. On this system, only the central tube of the gun is engaged to resist the longitudinal pressure on the breech. But, in the new gun, the central tube is supplemented by the rings in offering joint resistance to longitudinal pressure. For this object, the first line of hoops is replaced by a long sleeve, filleted at the ends, and shrunk on hot, in order to obtain longitudinal grip. The sleeve extends from the breech to the trunnion-ring, to which it transmits the longitudinal tensional force excited during the discharge of the gun.

By means of such a disposition, guns of equal resistance to bursting and unbreeching may be produced; whilst they are thinner and lighter, and of uniform thickness, more easily hammered and tempered. The new gun, entirely of steel, consists of an inner tube, strengthened by a long jacket. It is 19·32 feet long, or 35 calibres, and weighs 6·10 tons. The maximum charge which has been used is 71½ lbs. of powder, by which a projectile of 132 lbs. was discharged at a velocity of 2,073 feet per second, with a maximum pressure of 2,200 atmospheres. The projectile could, under these conditions, pierce a plate of iron 13¾ inches in thickness. At an angle of 35°, a range of 9 miles is reached.

A Table is given showing the respective performances of guns of several manufactures, from which the following comparative data are selected. The last column of the Table gives the ratios of the work accumulated in the discharged projectiles to the respective weights of the guns, the work-ratio of the 16-centimetre Spanish gun being taken as 1. It is designed to exhibit the relative efficiencies of the guns per unit of weight.

Usual Designation.	Bore.	Length.	Weight of Gun.	Weight of Projectile.	Weight of Charge.	Velocity of Discharge.	Work Accumulated.	Relative Ratios of Work to Weight of Gun.
	Inches.	Feet.	Tons.	Lbs.	Lbs.	Feet per second.	Foot-tons.	
16-centimetre gun, *Forges et Chantiers*	6·3	19·32	6·10	132	71·5	2,073	4,434	1·0
16-centimetre gun, French navy . .	6·45	15·97	5·02	99	49·5	1,984	3,048	0·80
6-inch Armstrong .	6·0	13·80	3·99	80	40·0	1,880	2,206	0·76
6-inch English, No. 2	6·0	13·80	4·05	100	40·0	1,682	2,210	0·75
„ No. 3	6·0	14·22	4·45	100	42·7	1,880	2,758	0·85
15-centimetre, Krupp	5·91	17·12	4·69	86	37·4	1,984	2,642	0·77
17-centimetre, Krupp	6·69	19·81	7·40	132	57·2	1,984	4,060	0·75

The gun-carriage of the 16-centimetre gun is constructed with a hydraulic brake, on Vavasseur's system. The recoil is limited to

27½ inches, the period of recoil is $\frac{21}{100}$ of 1 second, and the maximum velocity is 12½ feet per second. The return is regulated by means of the two brake-cylinders, filled with glycerine. The pressure in the breech mounts momentarily to about 460 tons, but the stress on the brake does not exceed 50 tons. The total weight of the mounting is—

	Tons.	cwt.	qrs.	lbs.
Carriage	1	11	2	20
Shield	0	12	1	17
Slide	2	17	3	15
Rivet-plate	1	10	2	23
	6	12	2	19

D. K. C.

A Cure for Steamed and Frosty Glass.

(American Machinist, No. 22, 1884, p. 5.)

A correspondent writes that a very thin coat of glycerine is a perfect cure for the trouble caused by water condensing on glass; he first noticed it used on a steam-gauge glass where the gauge leaked inside. Surveyors can use it on their instruments, and it does not injure the usefulness of field-glasses. It is very useful when applied to the windows of locomotive cabs in bad weather. The only disadvantage is that it collects the dust, and for this reason it should be applied very thin, and rubbed off and renewed when the dust collects to an injurious extent.

W. P.

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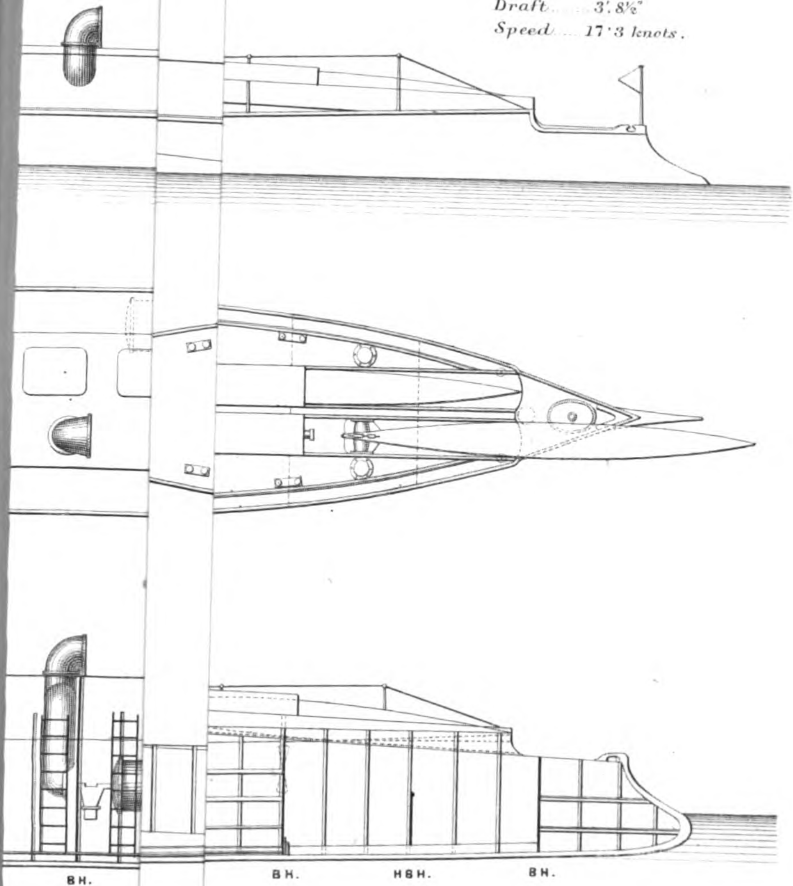

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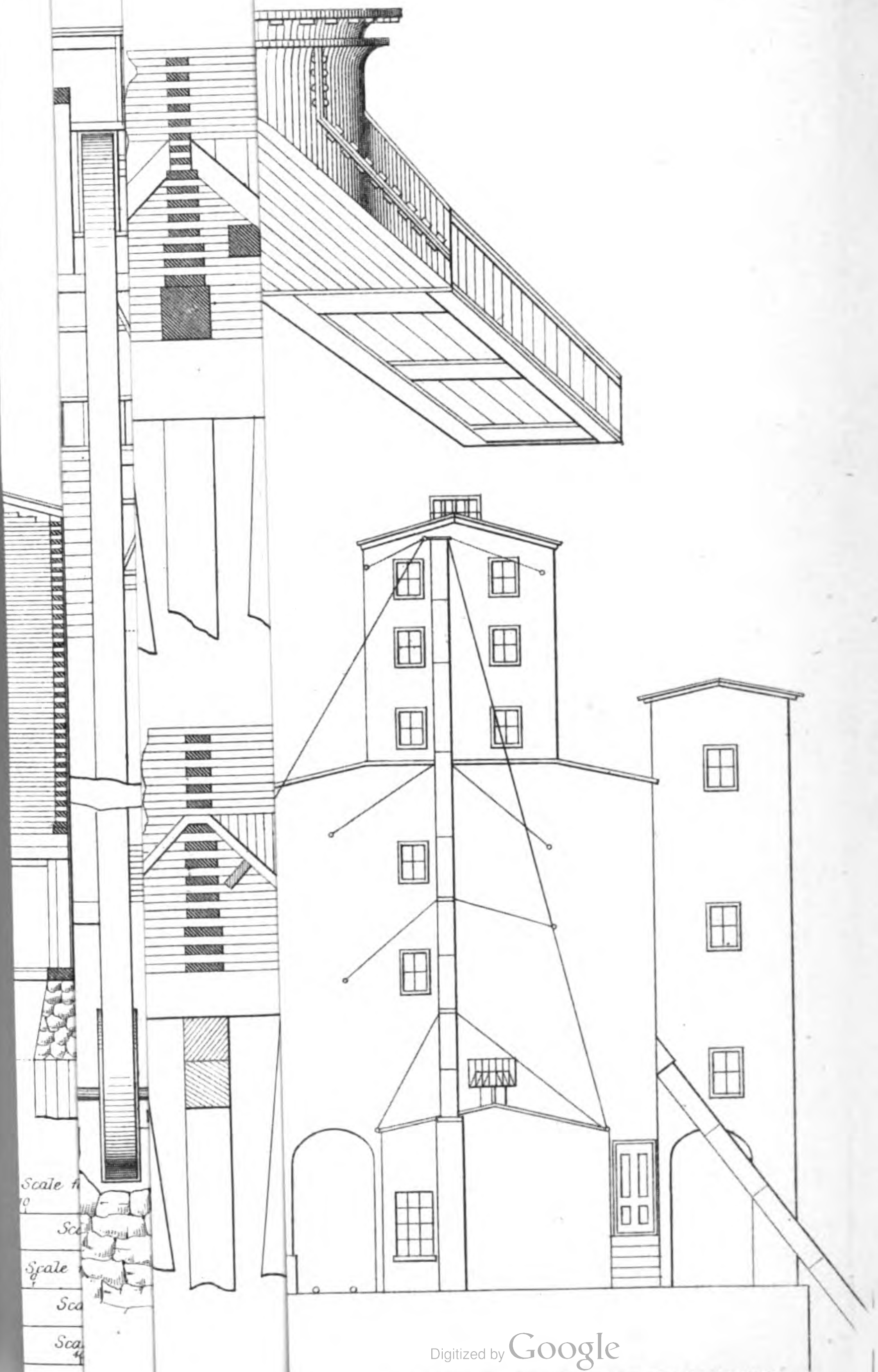
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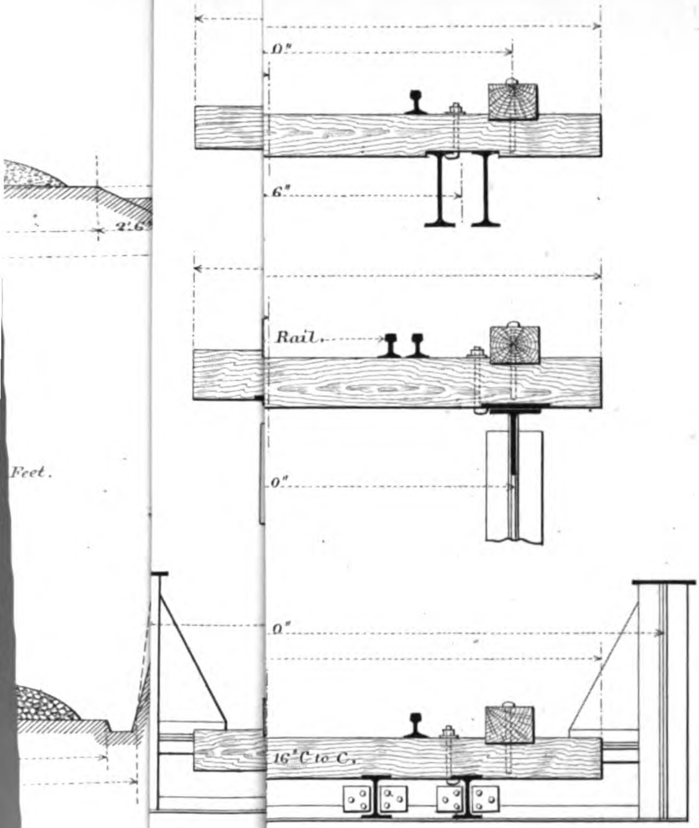

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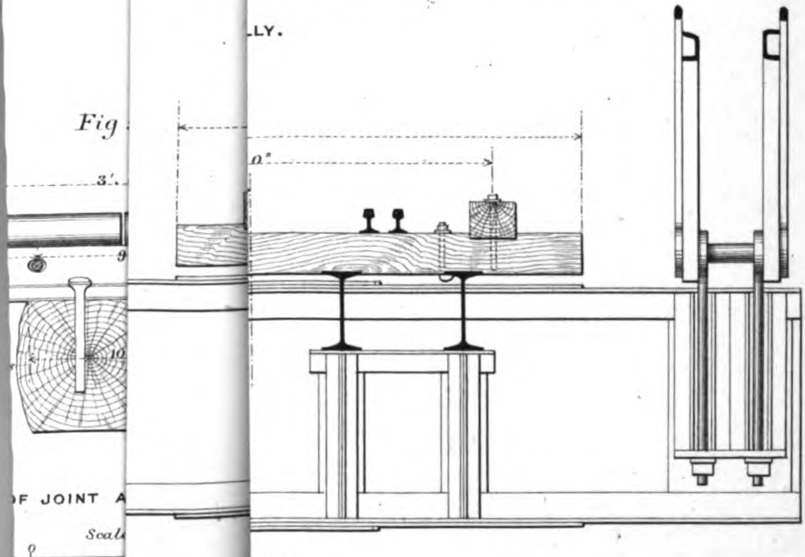

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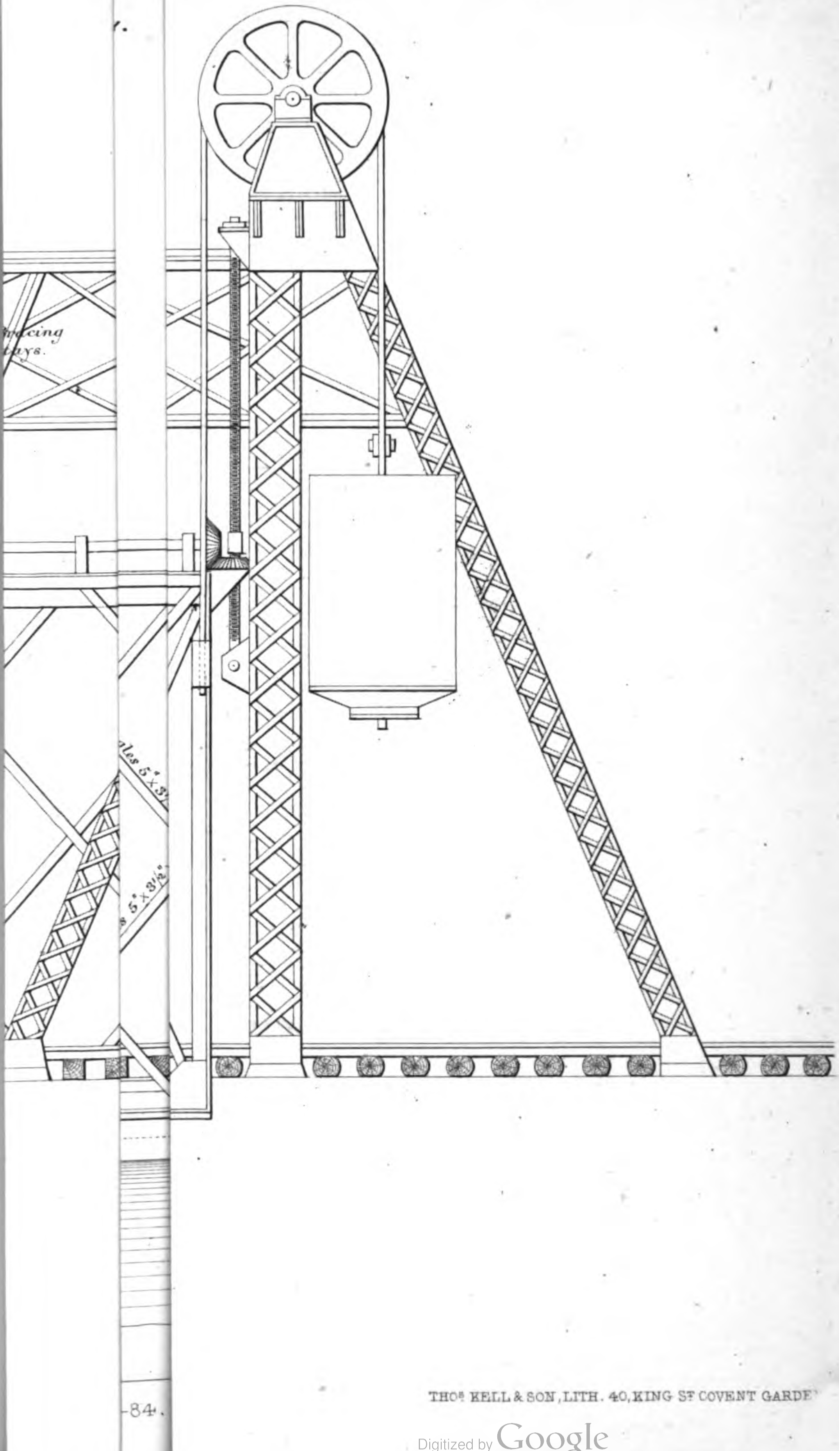
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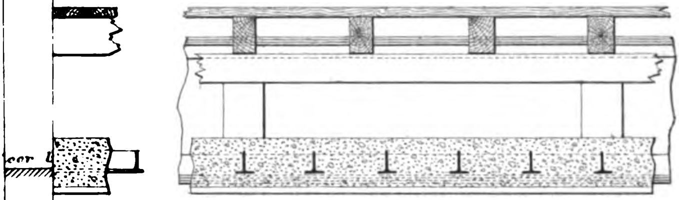

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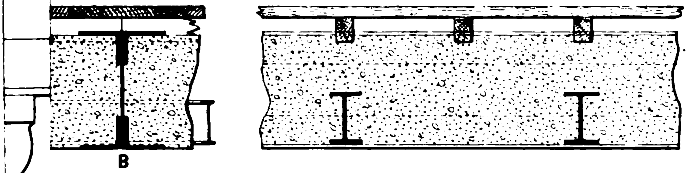

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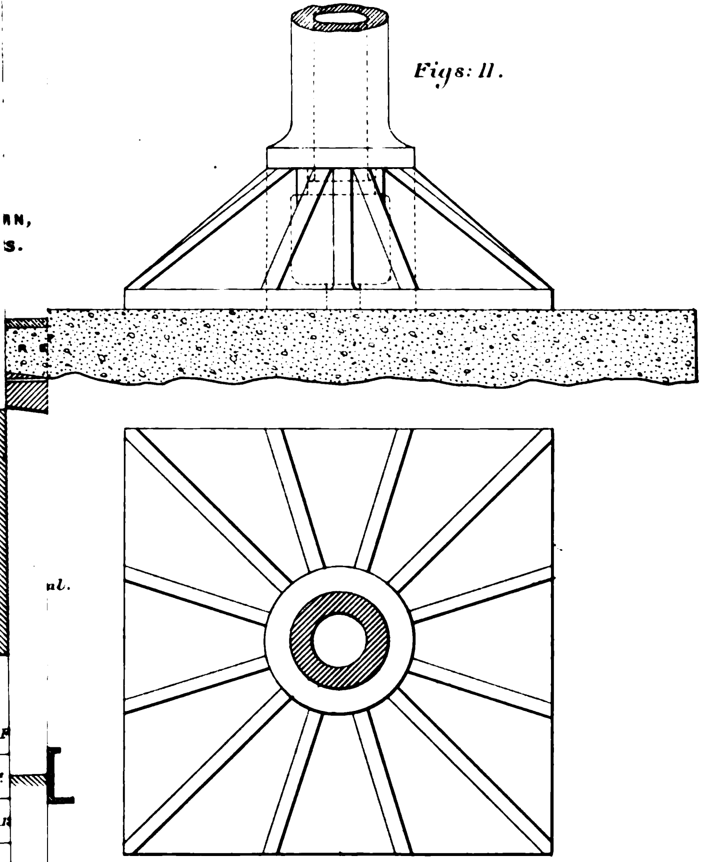

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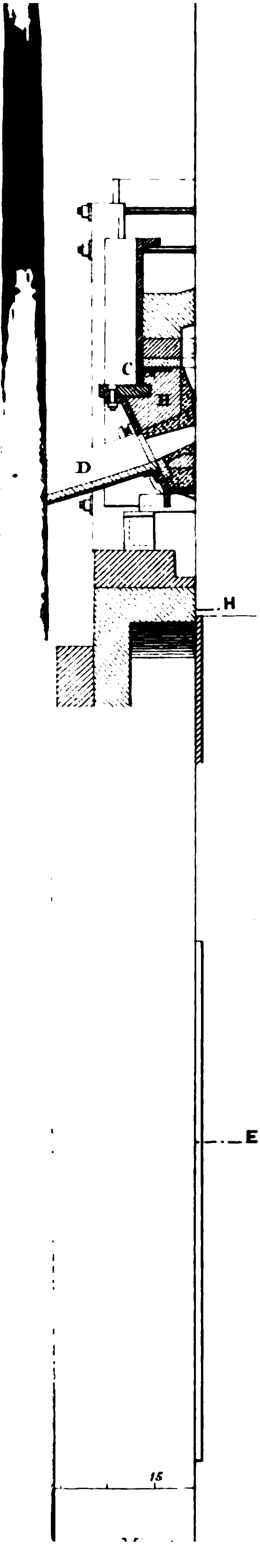

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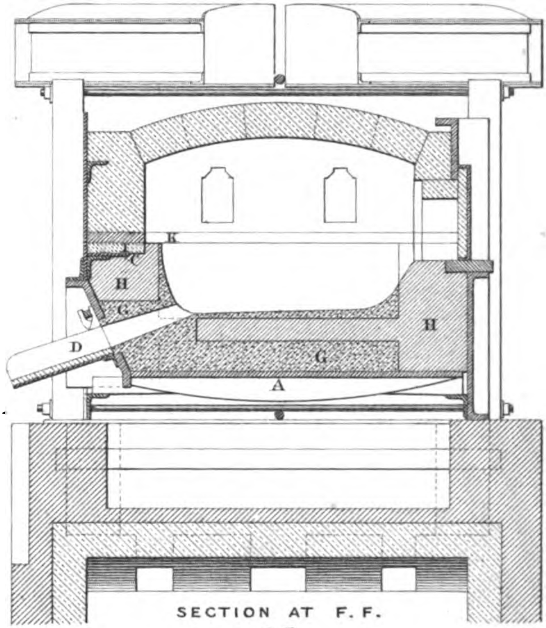

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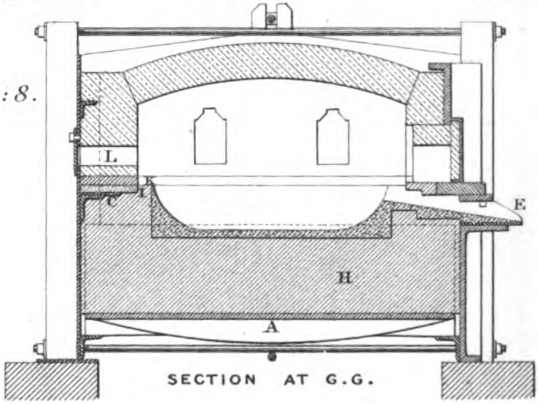

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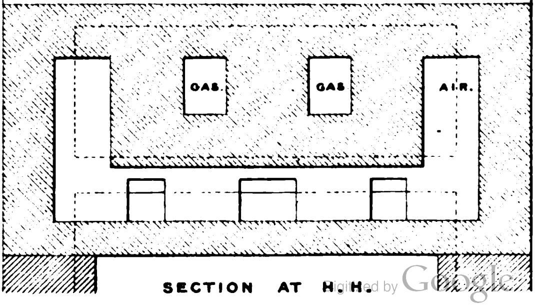

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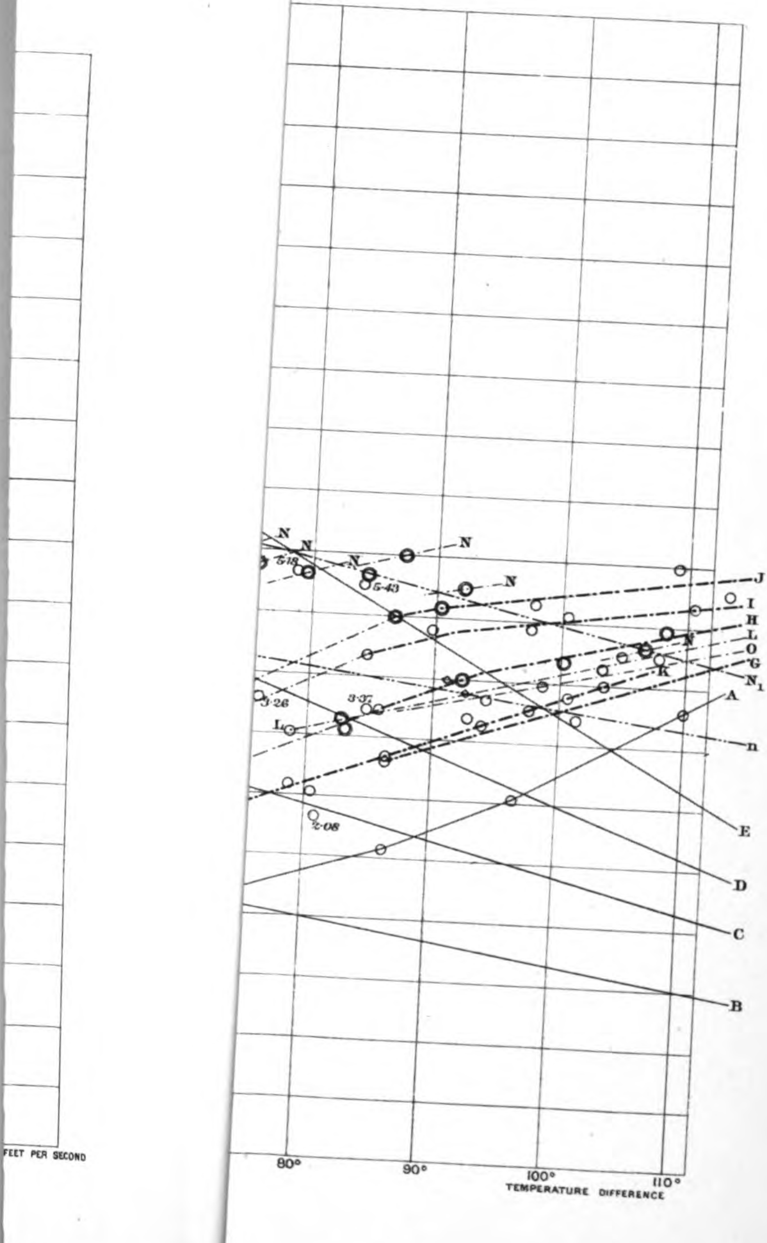

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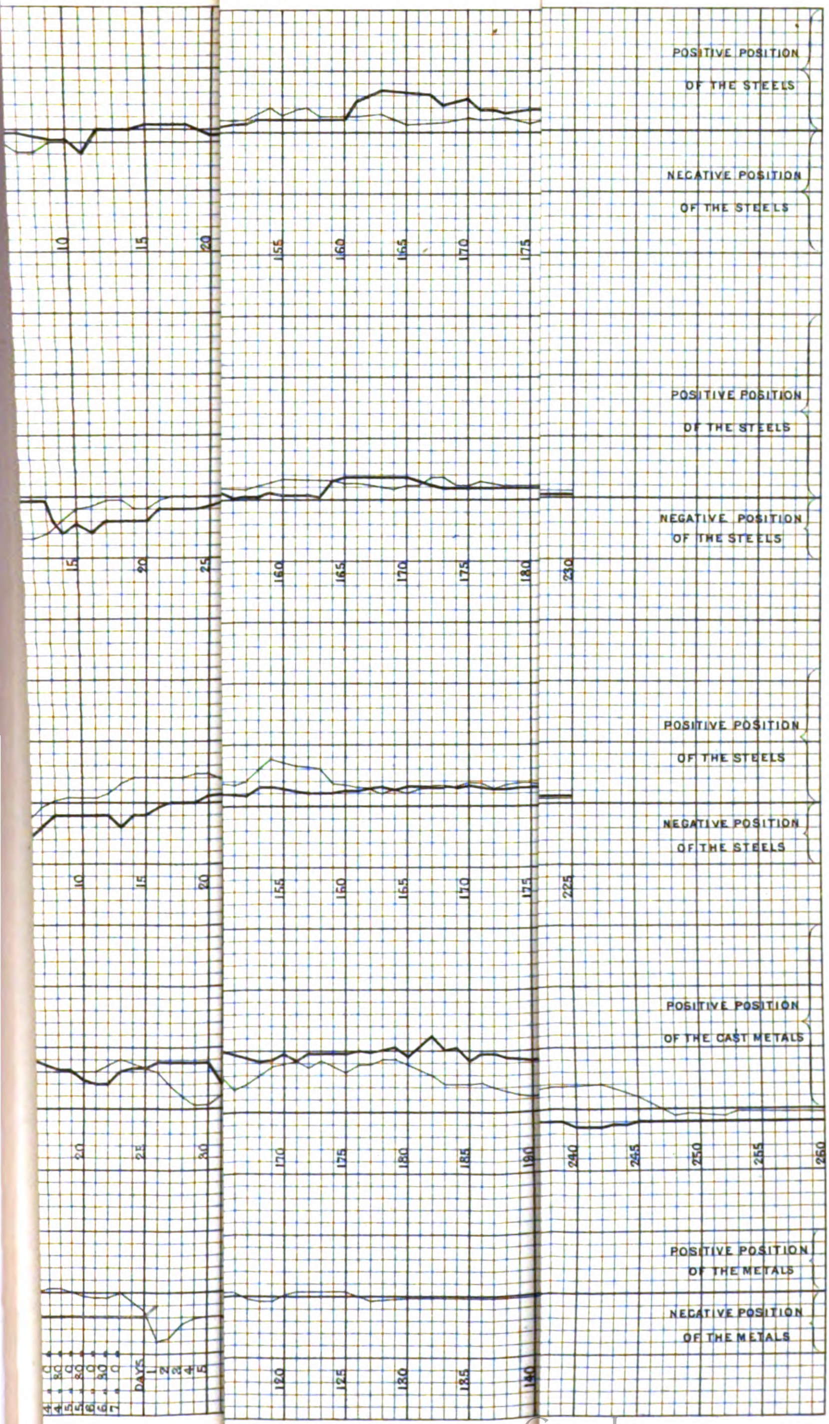

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